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AMERICAN JOURNAL OF SCIENCE

[THIRD SERIES.]

ART. I.—*Measurement of the Peruvian Arc;* by E. D. PRESTON.*

ONE hundred and fifty years have passed since Bouguer was making his observations in the measurement of the Peruvian arc. The geodetic science of to-day is so much occupied with the slight deviations of the surface of the earth from a strictly elliptical figure, that it is hard to realize that even in the last century it was an unsettled question whether the equatorial or polar axis was the longer.

A clock, having been carried from Paris to the equator, was found to lose two minutes each day. This fact was supposed to strengthen Newton's theory that the earth was an oblate spheroid. On the other hand, Cassini's surveys in France at the beginning of the last century, indicated a prolate spheroid. It was to reconcile these two determinations that the French Academy undertook the measurements of Meridional arcs; one on a frozen river in Lapland, the other above the clouds in Peru.

Parenthetically, it should be stated, however, that the Peruvian arc so-called, is not in Peru as defined by the geography

* Read before the American Association for the Advancement of Science, Toronto meeting, August, 1889. Published by permission of the Superintendent of the U. S. Coast and Geodetic Survey.

of to-day, but more than a hundred miles north of it in Equador. When the results of these two expeditions were made known, the scientific world accepted Newton's theory, and all later measures have only served to confirm it.

Let us pause just for an instant to examine the triangulation by Cassini, and the time determination by Richer. Looking at the data with our present knowledge of the accuracy attainable in the two kinds of measurement, it seems strange that the former could for a moment have cast doubt on the latter. In the first place, Cassini's results do not agree among themselves. He gives the following statement of the length of one degree in toises:

ϕ	τ
49° 56′	56970
49 22	57060
47 57	57098

Even were there no other reason for distrusting the observations, their disagreement would almost condemn them. We now know that the length of one degree in latitude 49° changes about ten toises per degree, so that the change of more than one hundred and thirty toises in a space of as many miles, indicates either some large error or a value for the earth's radii entirely incompatible with even the rudest observations. Besides, if the first difference were accepted, it would require the place of observation to be in a latitude very different from that known to have been the case. Therefore the triangulation in itself is not very trustworthy. Moreover any assumption in regard to the ellipticity of the meridian derived from measures not extending beyond two degrees, is extremely hazardous.

On the other hand, when we consider that Richer's clock lost two minutes daily, and that it must have been a comparatively easy matter, even at that time, to get differential time within a second, it is plain that the only source of error worth examining is that due to the change of the length of the pendulum. Barring accidents, and leaving to one side the effect of temperature, which must have been well understood and taken account of by the observer, the length could not have changed by nearly so much as its one-thousandth part, and as the time varies as the square root of the length of the pendulum, the time of one oscillation could not have been in error more than one-half this amount. Hence no error can be admitted that would materially change the result, and the pendulum work might have been accepted as demonstrating the oblateness of the figure. But the Academy resolved upon an independent determination, and the two expeditions were equipped.

Bouguer set out on May 6th, 1735, and after a journey of more than a year, arrived at his destination. The party was absent about nine years, but the triangulation and base measurements were executed between December, 1736 and August, 1739. Astronomical observations to determine the amplitude of the arc were made between July, 1741 and January, 1743, and the party arrived at the mouth of the Magdalena river on September 30th of the same year. The pendulum was swung at Porto Bello on the outward trip, and at Petit Goave, Hayti, on the return voyage. The results are incorporated in the account of the equatorial work. Three gravity determinations were made in Peru; at the sea-level, at Quito (9,374 feet elevation), and on the summit of Pichincha (15,564 feet). Magnetic observations were also carried on, and a general study made of the natural history and physical features of the country.

We must not lose sight of the fact that the work was undertaken to decide between the relative lengths of the earth's axes. Several methods of arriving at this result were therefore considered. It was once thought to supply sufficient data to decide the whole question by the equatorial observations alone: measuring for this purpose a degree of latitude and one of longitude in the same locality. But recognizing the fact that with the means at hand, the former would be subject to an error of 1/1500th part while the latter would be uncertain by about six times as much, the preference was given to the degree of the meridian. Measures had already been made in France, and from the nature of the involute curve, formed by the intersection of the earth's radii for any given meridian, it was admitted that combining the equatorial measures with those of a middle latitude, the error to be expected in the ratio of the two axes was only 1/1440th part; and that a combination with arctic measures would reduce the error to about two-thirds as much (1/2030). The errors attributed to accidental causes rest on the assumption that in each astronomical observation the observer is liable to be mistaken by three or four seconds of arc; and that in noting signals for longitude one second of time would be the error expected.

After the arc had been measured it became a matter of some difficulty to combine it with the French and arctic work. Every supposition made in regard to the meridian, supposing that it could be represented by an elliptical curve, seemed to do violence to the results of observation.

A combination was first made, using the arctic and equatorial arcs, the law being that of the square of the sines. This led to a ratio of 214 to 215 for the axes. Then when the middle arc was re-measured, and to the three meridian arcs a longitudinal one was added this ratio was changed, and 222 to 223 given. The formula was still that of the sines squared. Later, an error was discovered in this remeasurement. Picard had used a toise for his base measures, which was too short by its one-thousandth part. The introduction of this new value modified the result so essentially that the law previously adopted no longer satisfied the observations within admissible errors. The formula was changed to one where the increments of the length of the degree varied as the fourth power of the sines of the latitude, and a ratio of 178 to 179 was given for the length of the axes.

The introduction of a power of the sines higher than the square was done reluctantly. But it was found, that in order to represent the curve by the second power of the function, supposing the three arcs subject to the same error, it was necessary to increase the degree in France by sixty-nine toises, and diminish the other two by an equal amount. This would have re-established the ratio 214 to 215 and would have been nearer the truth, as we now know. But such large errors were not thought possible. In fact, reasoning from their accidental errors of observation, only an error of seventeen toises could be admitted for the middle degree, and forty-four for the equatorial one. This would necessitate subtracting one hundred and forty toises from the northern one, which seemed beyond all reason. The procedure, however, would make the meridian a perfect ellipse and give a ratio of 250 to 251.

When compared with Newton's theoretical value of the ellipticity, it was remarked that this erred in defect about as much as the previous conclusion had erred in excess. Therefore the observations left the choice of only two suppositions: either that of the fourth power of the sines, or that of some function of the latitude itself. The arc of longitude which had been measured was brought to bear on the decision, and it was found that the measure would, by the first solution, be in error by one hundred and fifty toises, whereas by the second the error would be reduced to eighteen. This decided the question and the law of the fourth powers, and the ratio 178 to 179 was adopted.

This is the result as given by Bouguer in his discussion of the Peruvian work. Of course it is far from being the truth; but the recapitulation shows to what extent the measures of one hundred and fifty years ago were defective, and gives an idea of the influence of this equatorial arc on the elements of the ellipsoid that are used in all geodetic computations of the present day. A later discussion improved this result, and now there are so many middle arcs entering with great weight on account of their increased accuracy, that the Peruvian arc has not the importance it once had. Notwithstanding, it is believed that a remeasurement would so modify it as to materially change the earth's ellipticity.

We now turn to examine the work more in detail. The first base was measured on the plain of Yarouqui, about fifteen miles east of Quito. Eight days were devoted to clearing the line. Its true direction was N. 19° 26′ W. Three wooden rods, each twenty feet long, with copper contact plates, projecting one and one-half inches at each end were used in making the measurement. The plates were so arranged as to make contact at right angles to each other. A rope was stretched for alignment, and the inclination of each rod was determined by means of a level. Twenty-five days were consumed in the work, which was exceedingly laborious because the rods were laid on the ground. This course was pursued on account of the violent wind. Two rods were always in position. The rear one, carried forward by Indians, was brought into contact with as little shock as possible; but with a heavy rod and in the hands of several untrained persons, it is difficult to see how shocks were avoided. An iron toise was carried along, kept in the shade, and comparisons were made always daily and sometimes oftener. The temperature and humidity of the air affected the wooden rods considerably. The work was begun from both ends and the parties compared their rods when they met in the middle. The south party, however, used tressels, and it was noticed that the effect of wind on the plumb-line, and the consequent error in the length of the base, would be in opposite directions for the two measures. In spite of this fact, and with the exceedingly rough method of making the contact, the two independent measures only differed by about three inches for a distance of more than six thousand toises, which is about $\frac{1}{150000}$th part. This is a degree of accuracy far beyond what we can reasonably expect in work of this kind, and there must certainly have been large compensating errors.

Base measures of the present time, with all our improved methods of dealing with the temperature, perfected contact slides, better ways of alignment, and more skillful manipulation of the bars by persons trained to the work, do not give much better results.

The actual measures gave somewhat less than 6273 toises for the length. It was estimated that the necessary corrections would increase this quantity, and in order to have their base an exact number of toises, one of the end marks was moved three inches and eight lines. It is hard to see what was to be gained by this. Of course the round number would be broken by the solution of the first triangle. Their own measures showed an uncertainty of several inches. Therefore, asserting that the base contained an exact number of toises within the thickness of a line, goes for nothing. Moreover the subsequent reduction gave a correction, different from what they had applied, so that the finally adopted length of the base was not 6274 toises, as they wished it to be, but four inches and one and one-half lines more than this.

The length of the straight line connecting the two extremities of the base was found by first comparing the actual measure with the line as traced on the ground, and then deducing the quantity sought from this last line. An approximate value for the base line substituted in the formula*

$$Z = \int \frac{c\,dx}{\sqrt{c^2 + (b + x)^2}}$$

gives the correction to reduce the actual measures to the ground line, considering it sensibly straight for each of the seven parts into which the whole base was divided. The absolute and relative heights of the extremities and the intermediate points were determined, which furnished the data for referring the ground line to the air line connecting the extremities. The result of the entire work was:

Ground line longer than actual measure......	1·52101
Ground line longer than air line..............	0·23100
Air line longer than actual measure..........	1·29301

The correction for temperature applied to the Tarqui base would indicate a coefficient of expansion of ·000015 for the wooden rods, which is between that of glass and brass but somewhat nearer the latter. But then not very much reliance can be put on the temperatures. That of the base of verification was only estimated.† and could not certainly have been known within several degrees. But the accordance of the results, errors of compensation being disregarded, would indicate that the temperatures were correct to within one-fourth of a degree. An examination of the record shows conclusively that this could not have been the case. On the other hand the Spanish officers correct the second base by about eight feet which would require a coefficient of expansion, based on the same difference of temperature, at least twice that given above.‡

* The earth's radius is c: x is the length of the line, and b an auxiliary constant.

† Mesure des trois premiers degrés du Méridien, par M. de la Condamine, Paris, 1751, p. 83; see also in this connection "Zeitschrift für Instrumentenkunde," August, 1885, p. 271, and "Resultate über die peruanische Gradmessung." Monat. Corresp., 1887, p. 240.

‡ Observaciones astronómicas y physicas hechas de órden de S. M. en los regnos del Perú. Por Juan y Ulloa. Madrid, 1748, p. 166.

It then appears that the temperature factor alone would give rise to uncertainties far greater than the difference between the two results.

The angles of the triangulation were measured with quadrants whose radii varied from two to three feet. Two telescopes were provided, a fixed and movable one, the whole instrument being universally mounted by means of two right-angled cylindrical elbows. Micrometers were here applied to instruments of this kind for the first time, and it is believed that Bouguer was the first who called attention to errors of eccentricity. As the limb of the instrument only included ninety degrees these errors could not be studied by the method now employed of comparing diametric readings throughout the entire circle. Independent measures of two known angles gave two equations, in which the known quantities were the errors, and a function of the angle itself, and the unknown quantities were the rectangular coördinates of the center of rotation, referred to the center of graduation as the origin. These coördinates being known from the solution of the equations, corrections applicable to any part of the limb could be calculated. Besides this, six or seven angles, which together closed the horizon, were measured. These were corrected for inclination and their sum compared with 360°. The error of closing was on the average about two minutes. Measures of equilateral triangles gave an additive correction of 20″ for an angle of 60°. Other combinations showed a correction of 40″ for 90°. The separate spaces of five degrees were examined by comparing with a known angle of this magnitude. A month was devoted to the study of the errors of the instrument.

With instruments capable of this degree of accuracy we cannot expect a close agreement between the measured and calculated base. They differ by about two feet. The triangulation is two hundred miles long and contains thirty-two principal triangles. But the result of the side computations from this principal network was twice modified. Once at the eighth triangle where some auxiliary figures gave a result different by two and one-half toises from the regular work, and again at the sixteenth, where results having a range of seven-tenths of a toise were obtained, from three different methods of deriving the same line. In both these cases the auxiliary work was combined with the regular triangulation, and the resulting line upon which the succeeding work depended, was changed by one and one-tenth toises in the first instance, and by three-tenths of a toise in the second; so that we should not be surprised at a much greater discrepancy between calculation and observation at the end of the chain of triangles. Bouguer shows that admitting an error of 15″ in each angle the accumulation of these throughout the entire work would produce an error in the second base of about twenty-five toises but adds that a certain compensation must be expected among so many errors.

After having finished the triangulation, astronomical observations were undertaken to determine the amplitude of the arc. They were made with a sector having a radius of twelve feet and a graduated arc of about three degrees. Although this is confessedly the weakest part of all the equatorial work, the methods employed show a keen appreciation of many sources of error. The limb was graduated by laying off an aliquot part of the radius as a chord. This was chosen with reference to the particular star to be observed, and the true zenith distance was found by applying to this known arc, a small micrometer correction. The modern work with the zenith telescope is but a repetition of this same principle ; for here the absolute zenith distance of the two stars is for the moment disregarded, but the excess of one over the other is measured, and applied to a function of their declinations, which are quantities determined by other investigations.

The precaution was taken in the Peruvian work to make part of the measures on the same star, and at the same time, at both extremities of the arc. This would eliminate any effect of uncertainty in the constants for precession, aberration and nutation, which were not, at that time, very well determined. But Zach has re-reduced the observations of 1742 and 1743,* and finds a difference of less than 1″ between the results for the simultaneous observations and that deduced from all the work during these two years. The instrument was reversed several times, thus giving values under different conditions, and it is said that no discordant observations were rejected.† The method of reversal is referred to as having been invented by Picard, and it is probable that this principle, now so often applied, and so essential in all instrumental work was here systematically used for the first time. Its effect in this case was to eliminate the eccentricity of the zero point of the micrometer.

The value of the micrometer was found from terrestrial measures, using the known length of portions of the base, and lines erected perpendicular thereto. The meridian was found by observing, at the moment of culmination, the direction of a beam of sunlight, admitted through a hole in the roof of the observatory. The method was supposed to give the true direction with an error not larger than one minute of arc.

* Ueber die Gradmessung am Aequator ; Monat. Corres. vol. xxvi, page 39.
† Figure de la Terre par Bouguer, Paris, 1749, p. 262.

The accuracy of the measurement of a star's zenith distance appears to depend principally on the stability of the limb of the instrument, and the ability of the observer to set the initial point of the arc in exact coincidence with the plumb-line. There seems to be nothing said as to how this was accomplished, but it is easily seen that it must have been a work requiring much care. The measures of the star's zenith distance are given to the nearest second. Indeed the three results for the arc's amplitude have a range of only three seconds. The following were the results from the three stars:

ε Orion	3° 7′ 1″
θ Aquilæ	6 59
α Aquarius	6 58

These are the results considered by the observers to be the best. They do not represent all the observations, but were selected on account of the favorable conditions obtaining at the time they were made. The sector had also undergone some improvement. But a mean value from all the results obtained at both stations gives 58″ which agrees more closely than one would expect from the range of the individual values. Zach estimates that the total error in the equatorial degree will not exceed fifty toises, thirty-eight being for the astronomical and twelve for the geodetic part of the work (Monat. Corres. 1807). This is based, however, on Bouguer's, and La Condamine's estimates of the accuracy of the astronomical work (page 251). Since the radius of the sector was twelve feet, one second on the limb would be about 1/1400th of an inch. To make a plumb-line, suspended freely, coincide with a mark on a scale at its side, to within less than this quantity, must have been a matter of difficulty. In this operation we have in all probability the source of the largest discrepancies.

The flexure of the sector was also studied. Experiments were made on an iron bar, from which it was concluded that the flexure varied as the fourth power of the length. It was found moreover that when the radius of the sector was placed horizontally its flexure amounted to one-twelfth of an inch. This was shown to be inappreciable when the inclination was only a few degrees, and when the objective of the telescope was attached to the center of the sector.

Azimuth observations were made at both bases and at three intermediate points. The agreement between the observed directions and those determined by triangulation is always within less than one minute; the discrepancy at the last base being forty seconds. The sun was invariably used, and the angle between it and a signal was measured with a quadrant. The errors in orientation, estimated liberally, will not change the total length of the arc more than a fraction of a second at each end. Therefore the question of azimuth is not one of vital importance.

We now come to perhaps the most interesting part of Bouguer's work. Not satisfied with investigating the exterior shape of the earth, he determined to study also its interior condition. It had been known for more than fifty years, that a pendulum oscillated more slowly at the equator than near the pole, and finding himself not only in a latitude where the force of gravity was the least, but also in a country where there were exceptional facilities for the study of this force at great elevations, he deemed it his duty to devote some time to the investigation of the subject. Of the two methods, either comparing the times of oscillation by the same pendulum, or comparing the lengths of two different pendulums, vibrating in the same time, he chose the former. In this he has been followed by all later observers. Of course his results cannot now be regarded as of very great value, both on account of the unsuitable methods and inferior instruments employed. But the work pointed in a certain direction, which has been confirmed in a general way by some more recent and accurate determinations. His method of getting time would not now be used in gravity observations, although modern instruments would increase the accuracy of the result several fold. It is doubtful whether, with his instruments, and often using single altitudes of stars, the time was correct within several seconds. About one and one-half hours may be taken as the duration of a swing, so that with the uncertain clock correction, and the short duration of the experiment, great discrepancies in the individual swings were unavoidable The pendulum was of an inaccurate type and its length was found by simply holding an iron bar by the side of a thread stretched by an ounce weight. Contact with the clamp above and the bob below was examined either by means of a magnifying glass or by the naked eye.

The individual results, for the length of the seconds pendulum at Quito, where the conditions were favorable, have a range of about 1/6000 part. This would correspond to discrepancies in the times of one oscillation of less than one unit in the fourth place of decimals. Under the circumstances the accordance is good.

The result was corrected for buoyancy and temperature. The former was here applied for the first time in pendulum observations. It was estimated that the density of the air on the top of Pichincha, was one eleventh thousandth of that of the metal composing the pendulum bob, and since gravity varies inversely as the length of the seconds pendulum, the length found was increased in this same ratio. No correction seems to have been made for the amplitude of oscillation.

When the necessary reductions were made it was found that gravity at the sea level, was diminished by 1/1331 part at Quito, and by 1/845 part on the summit of Pichincha. Since the distance from the earth's center had been increased in the first instance by its 1/2237 part and in the second by its 1/1348 part the results indicated a law, not very different from that of the inverse square of the distance. But gravity had not changed enough, in either case, to satisfy the law. The conclusion therefore was, that some influence, not exactly understood, increased the force of gravity in both cases. Naturally, attention was drawn to the high table land lying between the stations and the sea. It was estimated that the effect of this would be one-half of that of a shell of matter of the same density and thickness encircling the whole earth. Granting this, the diminution of gravity in passing from the sea to the summit would be

$$\frac{2h}{r}\left(1-\frac{3\delta}{4\Delta}\right)$$

where h is the height of the station above the sea, r is the radius of the earth and δ and Δ are the respective mean densities of the table land and earth.

Now this diminution was found by the pendulum to be 1/1331, which, compared with the above expression, leads to the conclusion that the matter composing the table-land has only about one-fifth the density of the earth. The result was something of a surprise at the time, and doubts began to arise as to whether the interior of the earth could be, as some supposed, a fluid mass surrounded by a thin shell. It could not be denied that the density of the surface was less than that of the interior, because it was shown that, in order that their densities be at least equal, the length of the second's pendulum must be in error by about one-thirtieth of an inch, which even with the rough method employed was too great an error to be admitted.

If the land lying between the upper station and the sea be regarded as a plain of infinite extent the same result ensues, and the formula deduced from this point of view is of somewhat simpler derivation. Clarke arrives at the same result by regarding the intervening matter as either a cone, cylinder, or segment of a sphere, where the horizontal dimensions are great compared with the vertical ones. In calculating some attractions in the Hawaiian Islands, the matter was treated rigorously as a cone, and the resultant attraction at the foot of the mountain, based on this value, agreed closely with that derived independently by the latitude observations and triangulation.

The value of the radius of the earth employed in the Peruvian investigation was about 12,000 meters too large. Introducing the value now accepted we get a density for the Andes somewhat greater. The change is in the right direction but it is not enough. The rocks in Peru probably have a density of about 2, or possibly less, and if the sea level is in error by one hundred toises, the pendulum work would give about this density for the underlying mass.

The method used in finding the absolute height of the base line, to which all the elevations were referred, was by triangulation. The results were roughly checked by the barometer. From Niguas, a point between Quito and the mouth of the Inca river, angles of elevation were taken to several mountain peaks, of which Pichincha was one. Niguas was also visible from a point near the sea level. The distances being approximately calculated, with some of the angles concluded, the elevations could be determined with some degree of accuracy. The last station was estimated from barometer readings to be about thirty toises above the sea. But that not much confidence could be placed in the instrument is plain from the fact, afterward stated, that weighing all circumstances it was concluded to fix the difference at forty or forty-two toises. The result was checked by a very rough estimate of the inclination of the river bed and the velocity of the current. Knowing the relation between the velocity and inclination at a point near the station, and determining the velocity farther down the stream, the inclination was calculated. Then from the measured horizontal distance and the inclination the vertical height resulted. It is evident that not much reliance is to be put in such a determination, but perhaps the error is considerably inside some others entering into the deduced height of Pichincha.

The angles of elevation were measured with a quadrant which might give results as much as 30″ from the truth. Then, as there were mountains back of the station, twelve thousand feet high in one case, and fifteen thousand in the other, the angles of elevation may have been in error in either case by the greater part of a minute. And errors from attraction would be accumulative, since Niguas is on the mountain flank. The distance from the sea to Ilinissa, with which Pichincha was connected, was found from the known difference of latitude and the azimuth. It seems therefore probable that the total elevation may have been in error by as much as fifty toises. This is not enough to bring the mean density of the Andes into tolerable accord with that of the surface rock.

It is difficult to accurately estimate the probable error of the distance between the two extremities of the arc, because sufficient data are not available. Take one-eighth of a foot, which is one-half the difference between the results, as the probable error of one measure of the base line. This is composed of errors in the lengths of the rods and errors of measures properly so called. The error in the entire base, as depending on the former, varies as the length, and as depending on the latter, as the square root of the length. Assume these to be equal. This would give for the uncertainty of one of the rods (twenty feet) 0·0004 inches or less than 1/500000th part, and for the uncertainty of making contact about 1/500 of an inch.

Either of these errors is not only much smaller than we can expect from work done under the circumstances, but they are actually less than are generally realized in modern measures. Therefore when we consider the means of comparison with the standard and the method of placing the bars on the ground, the close agreement must be considered entirely accidental, and in no wise to be taken as a criterion of the accuracy of the work.

Any error in the linear measure is transmitted through the triangulation and the probable error in the last side will depend on the average correction to a direction as determined from the shape of the triangles and their number. To this is to be added the error in the base, which transmits itself independently, and its effect depends on the relation between the base and the last side. The average direction error, resulting from joining points in a triangulation, is about twice as much as the average direction error arising from closing the horizon at any one point. Regarding the probable errors of the base and angles as differentials of those quantities, the uncertainty of any side may be computed by a formula involving these differentials and known functions of the angles.

Taking eight seconds as the probable error of an angle which is less than that estimated by the observer, we calculate the uncertainty of the last side, as depending on the angle equations alone to be slightly more than ten feet. This result is based on the formula,

$$r_{\circ} = ar \sin 1'' \sqrt{\cot^2 A + \cot^2 B},$$

which assumes that one of the angles in each triangle is a concluded one. The true probable error, where all the angles are measured, would be somewhat less than that given. Nevertheless, all the circumstances being considered we may assume the uncertainty of the last line to be not far from twelve feet. The chances are that this is an under-estimate. This error, as we have seen, is about that discovered near the middle of the chain, and which influenced all subsequent work by one-half its amount. The error in the base is now disregarded because, although it is much larger than the results of the measures would indicate, its effect on the last side would still be small in comparison with that resulting from the angle equations.

The astronomical observations agree among themselves, but it was not suspected at the time, that the mountains might affect the plumb-line by at least thirty times as much as the results were supposed to be in error. When the work was done instruments and methods had not been brought to that degree of perfection necessary to detect these small influences. Since then many striking cases have been brought to light, 22″ deviation having been noticed in India, 16″ in Russia, and 29″ in the Hawaiian Islands. In the example near Moscow there are no mountains to account for the phenomena, and the supposition is that the density of the underlying strata may be subject to great variations, or that large subterranean caverns may exist. Archdeacon Pratt has shown that small changes of density, if extended over a considerable area, may produce very perceptible deflections of the vertical. The Indian example is produced by the Himalayas. The Hawaiian is the result of the attraction of Haleakala, an extinct volcano ten thousand feet high.

When we consider that between the extremities of the Peruvian arc there is a continuous range of mountains, varying in height from nine thousand feet on the plateau of Cochesqui, to nineteen thousand at the summit of Chimborazo, and remember that the arc was terminated at a point where the elevation dropped suddenly several thousand feet, it is evident there must have been enormous differences between the astronomical and geodetic latitudes.

Judging from analogy with other cases, similar, either in the volume of the mountains or the density of the matter, it seems not unlikely that the amplitude of the arc may be in error by many seconds. Indeed if we take the data used in La Place's first discussion, the Peruvian latitudes should be changed by about 10″ in order to give an ellipticity conforming reasonably with our present value. And the required change shows that the plumb-line was drawn toward the mountains.

The errors in the measures of the two bases, in the triangulation, in the altitudes, or in the azimuths, could not have an influence at all comparable to this, so that a simple redetermination of the latitudes would very much improve the result. In fixing the figure of the earth an equatorial arc enters with great weight, and we find that in a combination by least squares of nine arcs used by La Place, an error of one minute in the amplitude of the equatorial arc would reduce the ellipticity to one-half its original value. This seems to be a great change for the supposed error, but it must be remembered that not only is the arc at the equator and therefore has great influence in the determination of the elliptic figure, but also that it is a comparatively short arc, and hence any error in the amplitude has a proportionately greater effect on the length of a degree deduced therefrom.

The individual influence of arcs where many enter into the determination should not, however, be overestimated. If we suppose arcs of one degree to be measured from the pole to the equator, say 10° apart, their weights in fixing the polar axis are approximately as the numbers 39, 43, 54, 70, 89, 111, 131, 146, 157, 161 and in the determination of the equatorial axis these same numbers apply in an inverse order. A curve plotted on rectangular coördinates, with the earth's radii and the above weights as arguments, has a point of inflection in middle latitudes, and since the ellipticity is unity minus the ratio of the two axes, middle arcs have very little influence on the ellipticity.

The pendulum observations indicate that the density of the mountains is about one-fifth the mean density of the earth. We may therefore assume that the Andes in the neighborhood of Quito are one-half as dense as the general surface of the earth; and if we take 15″ for the deflection at each end of the arc the ellipticity of the figure is changed by about one-fourth part of itself.

The effect of any change in an equatorial arc, on the figure of the earth, as deduced from the nine arcs above mentioned is easily found. The conditional equations are combined by least squares in order to find the values of M and N in the equation,

$$d = \mathrm{M} + \mathrm{N} \sin^2 l$$

where d is the length of one degree in latitude l, M is the length of one degree at the equator, and N is the difference in length between the equatorial and polar degree. The change in length of the equatorial degree will be given by differentiating an expression of the form

$$\frac{\Sigma\, b^2\, (\Sigma\, a - \Sigma\, ab)}{9\, \Sigma\, b^2 - (\Sigma\, b)^2}$$

where a is the independent variable. The degree being at the equator, the differential of $\Sigma\, ab$ is zero, and the change in the length of the equatorial degree from the solution of the normal equations would be about two-thirds the assumed linear error in the individual arc. Knowing the differentials of M and N the changes produced in the eccentricity and ellipticity are obtained without difficulty.

It is a singular fact that the first combination of the Lapland and Peruvian arcs gave a value for the ellipticity quite as near the truth as was deduced by La Place fifty years later, using the accumulated data furnished by improved instruments and methods. This is partly owing to the fact that the two arcs, having the greatest influence from their position and length, remained unaltered.

La Place's combination gives a value in excess, and the supposition of the plumb-line being deflected toward the center of the arc changes the value in the right direction. How much attraction should be allowed for it it is difficult to say as the configuration of the land is not known with sufficient accuracy. The indications are, that even admitting the small density of the mountains, the deflections are much larger than would be necessary to bring this arc into accord with the others, and give a value for the ellipticity called for by modern observations in middle latitudes. If we accept the data in La Place's first combination a deflection of eleven seconds at each end of the arc would be required for this purpose.

There seems to have been some compensation of errors, which has given the Peruvian arc a value conforming closely with our present spheroid. But its agreement in this respect is no excuse for not remeasuring it. The measures of the base line agree within a few inches, but no one who has examined the case believes that this is anything but an accident. The combination of this arc with some other recent ones in the determination of the figure of the earth, gives corrections for the equatorial latitudes even smaller than those required by points whose positions were determined with greater precision, and where the direction of the plumb-line is much less disturbed by attraction. These small corrections would probably not be confirmed by a new measure.

Notwithstanding, this work was well done considering the circumstances and the state of science at the time. Bouguer and his associates were scientific men who thoroughly understood the requirements of the case, and executed the work with the utmost fidelity. The necessity for a greater or less degree of accuracy, according to the kind of observations, and the bearing of each partial result on the final one was the source of constant study. Many principles of work here practiced for the first time have been adhered to by all later observers. But the advantage of repeating this work would come from the great improvements in instruments, and the consequent bringing to light of influences that were then unknown. Nothing at the time was known of spherical excess in geodesy. The theory of least squares was undiscovered, and the method of equal zenith distances had never been applied to the determinations of latitude. We now have also the compensating base apparatus and many perfected forms of the pendulum for the measurement of the force of gravity. In fact in every class of work the errors at present range from one-tenth to one-hundredth of what was then considered admissible.

Add to this that there is no check on the astronomical latitudes, which are doubly important on account of the shortness of the arc; that the elevation above sea is very uncertain; that their own observations show an uncertainty of seven or eight feet in the sides of the triangles, and that the arc enters into the determination of the ellipse with great effect owing to its geographical position, and it must be conceded that the geodetic science of to-day demands the re-measurement of the Peruvian arc. It is high time that the equatorial work be put on the same footing as the other data entering into this important problem.

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ART. II.—*Neutralization of Induction;** by JOHN TROWBRIDGE and SAMUEL SHELDON.

THE invention of the telephone drew attention to the extraordinary sensitiveness of Faraday's electrotonic state, and immediate attempts were made to construct induction balances, so called, which might serve for quantitative measurements. Thus we have Hughes's induction balance, which had its prototype in the balance described in Maxwell's "Electricity and Magnetism," vol. II, § 636, due to Felici,† and which differs from Hughes's balance merely in the employment of a galvanometer instead of a telephone. By substituting the latter instrument Hughes showed that great sensitiveness could be obtained, and even proposed to adopt an instrument for measuring minute amounts of impurities in coins arising from alloys.

The great difficulty, however, in the employment of Hughes's induction balance in quantitative work arises from the difficulty of getting a good minimum of tone in the telephone. The method that Hughes employed was, briefly, to employ four coils—two in a circuit through which an alternating current or an interrupted current was passed, and two other coils placed contiguous to the coils which were in the interrupted circuit, but in another circuit. By interposing a telephone in the last mentioned circuit, and

* From the Proceedings of the American Academy of Arts and Sciences.

† Nuovo Cimento, vol. ix, p. 345, 1859.

by properly placing the coils in this circuit with reference to those in the circuit through which the interrupted current was passed, a balance could be obtained, or an imperfect minimum of sound in the telephone, when the induction between the sets of coils was neutralized In order to obtain a standard, Hughes employed a wedge of zinc, which was thrust between one of the coils in the interrupted circuit and one of the coils in the telephone circuit, in order that the mutual induction between these coils might balance that arising between the other two similarly placed coils when a coin or sheet of metal was placed between these last mentioned coils. Other devices have also been employed by various investigators who have endeavored to use the apparatus for quantitative measurement. Alexander Graham Bell employed a modification of Hughes's induction balance for the detection of the presence of a bullet in the human body. In the form employed by him, one coil, which was a closely wound flat copper band, was made to slide over a similar one by means of a screw, one coil being placed in the telephone circuit and the other in a circuit containing a current-breaker. The induction arising from a similar pair of coils moved over a mass of metal like a bullet could thus be neutralized by this sliding coil arrangement. In no form, however, of Hughes's induction apparatus can one obtain a satisfactory minimum of tone in the telephone. There is never absolute silence, and no two observers can obtain the same point at which the sound seems to be a minimum. The failure to obtain this minimum is thus a radical defect in the instrument. It is doubtless very sensitive, but it cannot be called a quantitative instrument.

To remedy this defect, A. Overbeck and J. Bergmann* substituted an electro-dynamometer for the telephone, and worked out a method of obtaining the resistances of metals when they are in the form of thin circular plates. The standard of comparison they employed was a thin layer of mercury between disks of glass in a cylindrical reservoir. Preliminary investigations had shown the authors that a certain relation existed between the thickness and specific resistance and coefficient of induction of metals in the form of thin disks, which were placed between the coils of the induction balance. In a subsequent paper,† A. Overbeck gives the mathematical theory of the induction balance, which in the main is Maxwell's theory of current sheets applied to Arago's disk.‡ In employing the instrument to measure the effect of change of temperature on induction in copper plates, or, in other words, temperature coefficients, in which we found that Messrs. Overbeck and

* Annalen der Physik, xxxi. 1887, p. 792. † Ibid., p. 812.
‡ Maxwell's Electricity and Magnetism, vol. ii, § 668, *et seq.*

Bergmann had anticipated us,* we were led to adopt the following form of the instrument, which differed entirely from that of these authors. Four coils were employed, as in the Hughes form of instrument. One of the coils in the telephone circuit was fixed upon a horizontal axis which was at right angles to the axis of the coil. The coil could therefore be moved through all positions, from perfect parallelism to its neighboring coil in the interrupted circuit to a position at right angles to this coil. The horizontal axis was provided with an index arm which moved over a graduated circle. Calling θ the angle of inclination of the axis of the movable coil with the axis of the fixed coil in the interrupted circuit, and N the strength of the induction current in the movable coil, we have evidently, on the supposition that the strength of the alternating current remains constant,

$$N = \text{constant} \times \text{cosine } \theta.$$

When the axes of the coils are at right angles, cosine $\theta = 0$, and we should have silence in the telephone. Since adopting this arrangement we have discovered that Dr. Bowditch, of the Harvard Medical School,† has employed this arrangement of a movable coil placed in front of a fixed coil as a modification of Du Bois Reymond's apparatus for controlling induction currents so that they may be administered by known amounts for physiological purposes. In Du Bois Reymond's apparatus one induction coil was simply moved away from a fixed coil through which an interrupted current was passed, much in the same manner as the coils in Wiedemann's form of galvanometer are moved. Here no minimum could be obtained. In Dr. Bowditch's form of this apparatus, theoretically a minimum should be obtained, that is, when cosine $\theta = 0$, or when the axes of the coils were at right angles. An indication of an electrical current is obtained even when the axes of the coils are at right angles, on account of the windings of the coil not being perfectly at right angles to those of the stationary coil.

That no minimum should be obtained when the axes of the coils are at right angles, and when the induction arises from all parts of the circuit, is evident upon an elementary consideration of the subject. We have to deal in this form of instrument with the mutual induction which arises between the fixed coil and the movable one, and also with the self-induction which arises between the spires of the movable coil in the telephone circuit. The mutual induction can be reduced theoretically to zero by placing the movable coil of the telephone circuit at right angles to the fixed coil. The self-induction can

* Annalen der Physik, xxxvi, 1889, p. 783.
† Proc. Am. Acad., vol. xi, p. 281.

be estimated as follows. Taking Maxwell's discussion for the induction between parallel circuits of radii A and a, we have the coefficient of mutual induction,

$$M=\iint\frac{\cos e\, ds\, ds'}{r}.$$

Projecting one circle upon the plane of the circle of greater radius, A, we have

$$M=\int_0^{2\pi}\int_0^{2\pi}\frac{Aa\cos(\varphi-\varphi')\,d\varphi d\varphi'}{\sqrt{A^2+a^2+b^2-2\,Aa\cos(\varphi-\varphi')}}.$$

Making b the distance between the planes of the circles $=0$, we pass from the case of mutual induction to that of self-induction between two spires of a coil which may be considered approximately circular. The form of M adapted for calculation is then

$$M=4\pi\sqrt{Aa}\left\{\left(c-\frac{2}{c}\right)F+\frac{2}{c}E\right\},$$

where $c=\dfrac{2\sqrt{Aa}}{(A+a)}$, and F and E are complete elliptic integrals to modulus c.

If we make $A-a=D$, or $A=D+a$, in which D is the distance between the spires at which the self-induction becomes insensible, the most perfect minimum can be attained. We have found that copper wire of 2 mm. diameter, wound in a flat loose spiral, the spires of which from center to center of the wire are 4 mm. apart, gives no sensible self-induction for spirals of eight to ten spires. On turning a movable coil of this form so that its axis may be perpendicular to the axis of the fixed coil, a perfect minimum can be obtained. A slight movement to the right or left of this position is quickly made evident by the note of the interrupted circuit which is heard in the telephone. It is evident that, if four coils are employed, as in Hughes's form of induction balance, the two coils in the telephone circuit should be wound in the manner we have indicated, to avoid self-induction. On placing a plate of metal between one set of the coils of this balance, the movable coil no longer gives a minimum at the position where its axis is at right angles to that of the fixed coil, but at some point removed a few degrees from this. By placing a mirror upon the movable coil, and by observing its deflection with a telescope, a greater refinement of reading is possible.

This instrument in its modified form suggests the possibility of neutralizing induction upon telephone circuits. The extension of the various systems for transmitting power by electricity, especially the electric car system, has led to great disturbances in the telephone circuits. These disturbances are due both to leakage from the power circuit into the telephone circuit, since the earth is used partially by the electric power companies in their return circuits, and to actual induction. The best remedy for these disturbances is doubtless the adoption by either the power companies or the telephone companies of entire metallic circuits, in which the earth plays no part. If this is not possible, a system of neutralization for the inductive disturbances might be adopted as follows. Let a shunt circuit from the electric light wire or the wire carrying the current for motors be led into a station through which also passes the telephone wire. The resistance of this shunt or derived circuit can be made suitable for the purpose. In all cases it reduces the resistance of the main line, and is therefore not prejudicial. On this shunt can be arranged a fixed coil, and on a neighboring telephone wire a movable coil of no self-induction. Let this movable coil be placed in front of the fixed coil in the motor circuit, and let it be turned until the mutual induction between it and the fixed coil neutralizes the induction produced at all points along the telephone circuit. Each telephone wire would need its movable coil, and to every movable coil would correspond a fixed coil in the shunt of the motor circuit. The operator at the central station could adjust the movable coils until the disturbances arising from induction at various points along the line are neutralized.

ART. III.—*Divergent Evolution and the Darwinian Theory*; by Rev. JOHN T. GULICK, Ph.D.

IN a paper on *Divergent Evolution through Cumulative Segregation* (Linnean Soc. Journal, Zoology, vol xx, pp. 189–274), I have endeavored to show that selection, whether natural or artificial, is a process that has no tendency to produce divergent evolution, unless different sections of one original stock are subjected to different forms of selection, while at the same time some cause prevents free crossing between the different sections. We now inquire whether Darwin has made us acquainted with any cause or combination of causes that, without the aid of man, produces diversity of selection and at the same time the independent generation of the different classes of variations thus preserved.

Darwin discusses the causes of natural selection more fully than the causes of diversity of natural selection. He does not speak of uniformity and diversity of natural selection, but of the individuals of the same species living under the same external conditions as being modified in the same way, and of those living under dissimilar external conditions as being modified in different ways. Again, he speaks of "the divergent tendency of natural selection," resulting from "the principle of benefit being derived from divergence of character," as explaining divergence of character in the members of one species competing with each other on a common area. How the contradictions in the two statements are to be reconciled, and how, in the second case, the unifying influence of free-crossing is prevented, he does not show, so far as I can discover. As the subject is of the highest importance in the explanation of divergent evolution, and as it is specially desirable to get as clear an understanding as possible of Darwin's method of explanation, I shall consider his reasoning somewhat fully.

Some degree of Local Separation under Different Environments.

Darwin often speaks of the influence of crossing in retarding or preventing the formation of new races and species; but, from the following extracts from his Origin of Species, it will be seen that it is not quite so clear what combination of causes he considered necessary for the production of two or more species from one original species. The obscurity in his statements results, I think, from the fact that "a new species" may be one that has been formed by monotypic transformation, the old form disappearing with the production of the new, or it may be one that has arisen through polytypic transformation which is the modification of one branch of the species, while other branches remain either unmodified or modified in other ways. For the formation of a new species, in the former meaning of the word, he evidently did not consider it necessary that the species or any part of it should enter a new environment, or that crossing should be prevented. But did he not consider both these conditions necessary for the formation of two or more species from one original species?

He says "Intercrossing will affect those animals most which unite for each birth and wander much, and which do not breed at a very quick rate. Hence with animals of this nature, for instance birds, varieties will generally be confined to different countries; and this I find to be the case. With hermaphrodite organisms which cross only occasionally, and likewise with animals which unite for each birth, but which wander little and can increase at a very rapid rate, *a new and improved variety might be quickly formed on any one spot*, and might there maintain itself in a body and afterward spread, so that the crossing would be chiefly between *the individuals of the new variety living together in the same place*. . . .

"Even in the case of animals which breed slowly and unite for each birth, we must not assume that *the effects of natural selection* will always be immediately overpowered by free intercrossing; for I can bring a considerable body of facts showing that within the same area, varieties of the same animal *may long remain distinct*, from haunting different stations, from breeding at slightly different seasons, or from varieties of the same kind preferring to pair together. . . .

"Isolation, also, is an important element in the changes effected through natural selection. *In a confined or isolated area, if not very large, the organic and inorganic conditions of life will be almost uniform: so that natural selection will tend to modify all the varying individuals of the same species in the same manner.* Intercrossing with the inhabitants of the surrounding districts will, also, be prevented. Moritz Wagner has lately published an interesting essay on this subject, and has shown that the service rendered by isolation in preventing crosses between newly formed varieties is probably greater even than I have supposed. But from reasons already assigned, I can by no means agree with this naturalist that migration and isolation are necessary for the formation of new species." [Origin of Species, fifth edition,* Chapter IV, Section on "Circumstances favorable for the production of new forms through Natural Selection."]

Again in the same chapter, in the section on "Various Objections," in answer to the question, "How, on the principle of natural selection, can a variety live side by side with the parent-species?" he replies, "If both have become fitted for slightly different habits of life or conditions, they might live together, though in the case of animals which freely cross and wander much about, *varieties seem to be almost always confined to distinct localities.* But if we put on one side polymorphic species, in which the variability seems to be of a peculiar nature, and all mere temporary variations, such as size, albinism, etc., the more permanent varieties are generally found, as far as I can judge, *inhabiting distinct stations, high land or low land, dry or moist districts, or distinct regions.*"†

In the portions of these passages which I have distinguished by italics, Darwin seems clearly to maintain that for the formation of coexistent permanent varieties some degree of local separation is necessary. I therefore conclude that when he says, he cannot regard migration and isolation as necessary for

* The same passages occur in the sixth edition, pp. 80, 81.

† In the sixth edition this passage will be found, slightly modified, in Chapter VII, p. 169.

the formation of new species he intends to express, in opposition to Moritz Wagner, the opinion that a species may be transformed into a new species without leaving its original locality, but that he does not intend to say, that two or more divergent species can arise in the same locality from the same stock. If I interpret him rightly he considers the partial separation described in the first of the paragraphs just quoted as sufficient to allow of the formation of divergent species, when the external conditions of the separate districts are sufficiently different and sufficiently permanent to secure long continued divergent natural selection. That the second paragraph is to be interpreted in accord with this meaning I judge from the fact that natural selection is mentioned here as the cause of the divergence which crossing tends to overpower, and in the third paragraph, uniformity in the environment is represented as ensuring uniform natural selection. The varieties that are restrained from crossing with each other by diverse times and habits of breeding, he must regard, sometimes as slightly divergent forms tending to disappear under the pressure of uniform natural selection, and therefore never becoming separate species though one of them may prevail and be established as a new species; and sometimes as forms that are becoming more and more divergent, because they have found their way into districts or stations where they are somewhat separated from each other, and where the conditions are somewhat different, and the natural selection, therefore, somewhat diversive.

If this is not his meaning, if he intends to teach that forms arising in one place and not locally separated from each other can continue to diverge till they become separate species, how can he say on the next page that forms isolated in a small area, being exposed to uniform conditions, would be modified by natural selection in a uniform manner. He evidently does not intend to be understood as teaching that in these cases mentioned in the second paragraph there is a cause of divergent evolution which produces separate varieties and species in spite of the unifying influence of natural selection resulting from uniform conditions.

Darwin's Theory of Natural Selection through the Advantage of Divergence of Character.

There is however, one passage in the "Origin of Species" which may be interpreted as assigning a cause for divergence of character in representatives of the same species that are surrounded by the same environment. These are the words; "Only those variations which are in some way profitable will be preserved, or naturally selected. And here the importance of the principle of benefit being derived from divergence of character comes in; for this will generally lead to the most different or divergent variations being preserved and accumulated by natural selection." (Origin of Species, Chap. IV, first page of the section on the "Probable Results of the Action of Natural Selection, through Divergence of Character and Extinction in the Descendants of a Common Ancestor." In the sixth edition, this passage occurs on pp. 90–1). The connection in which this passage stands seems to indicate that "the benefit derived from the divergence of character" is considered the cause of "the most different or divergent variations being preserved and accumulated by natural selection," even in the case of the representatives of the same species that are competing with each other on the same area, and are in no way prevented from intercrossing. It is therefore necessary to show the difficulties that beset such a theory, especially if we adhere to the more general theory, that diversity in the kinds of natural selection affecting a species must be due to differences in the environments by which it is surrounded.

In the first place, natural selection, which is the superior propagation of those best adapted to the environment, prevents the interbreeding of the adapted forms that propagate with the unadapted that fail of propagating; but it can never prevent the interbreeding of those forms which through different kinds of adaptation to the environment survive and propagate, and, therefore, it can have no influence in producing accumulated divergence, unless it is supplemented by some segregative principle that prevents the different kinds of adaptations from being interfused. In the second place, as long as we follow Darwin's explanation of the causes of natural selection, we must hold that the representatives of one species while surrounded by the same environment, whether prevented from intercrossing or not, will, through the uniform action of natural selection, be modified in the same way, if at all, and, while surrounded by distinct and dissimilar environments, will be modified in divergent ways; but, in this latter case, as they will be prevented from competing with each other by occupying different areas, they can derive no advantage from divergence of character through its preventing competition, therefore the divergence that follows must be attributed to some other cause. In other words, the advantage attributed by Darwin to divergence of character is freedom from competition, through diversity of adaptation, and, as some degree of prevention of crossing is necessary for permanent difference in adaptations, the advantage cannot be secured unless there is some cause preventing the crossing of the divergent forms. Now, the prevention of crossing, if it ever arises, will be secured either while the individuals that are prevented from interbreeding are occupying the same limited area and exposed to the same environment, or while occupying distinct areas and exposed to either the same or different environments. In the first case, we are told by Darwin, that exposure to uniform conditions "will tend to modify all the varying individuals of the same species in the same manner." In the second case, as the sections of the species that are prevented from crossing occupy separate areas, the advantage of freedom from competition is already secured without divergent adaptation, and there can be no further advantage of that kind.

Again, it is not difficult to show that divergence is in itself no benefit, for multitudes of more divergent forms fail, leaving the field to less divergent ones. This is generally true of monstrosities, and frequently true of other kinds of variations. Neither can it be claimed that freedom from competition is an advantage unless it results in freer access to unappropriated resources, and this advantage is most frequently gained by migrating into a locality presenting the same environment but not previously occupied by the species. In this last case, the access to unappropriated resources does not depend on new adaptations; and, as any new adaptations that might bring advantage to the representatives of the species in one district would be of equal advantage in the other district, no divergence of character could be advantageous. It is this impossibility of advantage in divergence of character in portions of a species exposed to the same environment which leads many naturalists to maintain that isolation does not tend to produce divergence unless accompanied by exposure to different environments. But their reasoning is inconclusive inasmuch as they have never shown that divergence depends on its being advantageous. In my study of Sandwich Island molluscs I have found very strong reasons for believing that divergence may arise in the representatives of one species during exposure to the same environment, producing not only non-adaptive, but also adaptive differences. But whether adaptive or non-adaptive, whether due to natural selection or to some other principle, differences that arise under the same environment cannot be advantageous differences, and the divergence through which the differences are reached is not advantageous divergence. It seems to me evident that, neither is divergence always advantageous, nor is the advantage of access to unappropriated resources necessarily dependent on divergence; that, neither does the accumulation of divergence depend on its being advantageous, nor is advantageous divergence always accumulated.

Darwin's Theory that Exposure to Different Environments is Essential to diversity of Natural Selection.

Diversity of natural selection in different portions of the same species depends upon diversity in the relations of the different portions to the environment. Now, observation shows that cumulative diversity in the relations of the species to the environment may be introduced, (1) by dissimilar changes in the environment presented by the different areas occupied by the different portions; (2) by different portions of the species entering different environments; or (3) by dissimilar changes in the habits of the different portions of the species in using the same environment. Certainly in this third class of cases, if not in the other classes, without prevention of free crossing between the different portions, there can be no cumulative diversity in relations to the environment, and therefore no cumulative diversity in the natural selection; and without the same condition, there can be no accumulation of divergent effects of natural selection, in any case. Darwin, however, forgetting the possibility of divergent changes in the habits of isolated portions of a species exposed to the same environment, maintains that exposure to different environments is essential to diversity of natural selection and to divergence. Without change in the climate, soil, or organic forms lying outside of the species there is, according to him, nothing to produce modification.

"If a number of species after having long competed with each other in their old home were to migrate in a body into a new and afterwards isolated country, they would be little liable to modification; for neither migration nor isolation in themselves effect any thing. These principles come into play only by bringing organisms into new relations with each other, and in a lesser degree with the surrounding physical conditions."—[Origin of Species. On the 4th and 5th pages of the first chapter on Geographical Distribution.]* "Each separate island of the Galapagos Archipelago is tenanted, and the fact is a marvelous one, by many distinct species; but these species are related to each other in a very much closer manner than to the inhabitants of the American continent, or of any other quarter of the world. This is what might have been expected, for islands situated so near each other would almost necessarily receive immigrants from the same original source. and from each other. But how is it that many of the immigrants have been differently modified, though only in a small degree, in islands situated within sight of each other, having the same geological nature, the same height, climate, etc.? This long

* See ed. 6, p. 319.

appeared to me a great difficulty; but it arises in chief part from the deeply seated error of considering the physical conditions of a country as the most important; whereas it cannot be disputed that the nature of the other species with which each has to compete, is at least as important, and generally a far more important element of success. Now if we look to the species which inhabit the Galapagos Archipelago, and are likewise found in other parts of the world, we find that they differ considerably in the several islands."—[Origin of Species, near the middle of the second chapter on Geographical Distribution.]*

The implication in both these passages is that if the representatives of the same species are surrounded by the same organic forms as well as by the same physical conditions in isolated countries, they will not undergo divergent modification. This is in complete accord with the third paragraph quoted near the beginning of this paper from the 4th chapter of the "Origin of Species."

Divergent Forms of Sexual Selection.

In the passages last quoted there is no mention of any exception to the principle that difference in external conditions is necessary to divergent evolution. No suggestion is given that through the action of sexual selection divergent species may be produced that are not at all dependent on differences in the environments, still there can be no doubt that this was Darwin's view. Though he does not directly discuss this problem in any passage I have been able to discover, he clearly expresses the opinion that the differences between the different races of man, and between man and the lower animals, are in no small degree due to sexual selection, and he never speaks of difference in sexual selection as depending on difference in the environment, though, at the close of the twentieth chapter of "The Descent of Man," he speaks of sexual selection in man as having probably "exaggerated" the "characteristic qualities" "which are of no service to" the tribes and races that possess them. The differences, however, in the races of man are attributed to sexual selection, not because of any lack of difference in their environments, but because the characters in which they differ do not seem to him to be related to the environment. The color of the skin, hair, and eyes, and the different forms of the head and face do not seem to be adapted to different conditions in the environment, while they are undoubtedly occasions of attraction or aversion for those seeking partners. He has not, however,

* See ed. 6. p. 355.

shown whether the change of taste precedes the change of form and color, or the reverse. Differences between the sexes of the same species in secondary sexual characters, are for weighty reasons attributed to sexual selection; but he does not show how this divergence between the sexes leads to the production of new species. This production of difference of character between the sexes, being in no way dependent on the prevention of crossing between the divergent sexes, must be a wholly different process from the production of races and species, which is absolutely dependent on prevention of crossing between the divergent races and species. There is, nevertheless, every reason to believe that when the representatives of a species capable of sexual selection are for many generations separated into groups that never cross, diversity of tastes is one of the forms of diversity that inevitably arises; but that the psychological divergence is the cause of the other correlated divergences is not so certain. The theory of divergence in races because of divergence in the forms of sexual selection seems to rest on the assumption that a psychological divergence may be accumulated and rendered permanent in a new and definite form without being subjected to selection; but if this is true of a psychological divergence, why may it not be true of any form of divergence? The difference in the ideals of beauty in different races is as important as difference in the skin and hair; and in accounting for the origin of races, it is quite as important to account for the former as for the latter; any theory that simply attributes the difference in the color of the skin to difference in the ideal of beauty, will be met by the suspicion that the difference in the ideal was preceded by the difference in the color. My own strong conviction is that the true explanation is equally applicable to either set of phenomena.

Darwin's reference to the Causes which Check the Crossing of Varieties.

In the second paragraph quoted from Darwin at the beginning of this chapter we find mention of three causes that may for a long time prevent the members of the same species from freely intercrossing while occupying the same area; but subsequent statements, in the same and the three succeeding sections, show that he regarded geographical and local separation as the forms of separate breeding that are most favorable to the production of new species. Moreover, in the two sections relating to "Divergence of Character," he seems to maintain that the prevention of intercrossing is not a necessary condition for divergence of character in members of the same species that are competing with each other.* In chapter XVI, of his "Variation under Domestication" several causes that interfere with the free crossing of varieties are enumerated; but they are nowhere recognized as essential factors in the evolution of divergent varieties and species, without which diversity of natural selection would be of no avail, and with which divergence will take place though there is no change in the environment. They are looked upon as characteristics in which many varieties more or less resemble species; but they are regarded as the results rather than the causes of divergent evolution.

Conclusion.

We, therefore, find that though Darwin has not recognized segregation, which is the independent propagation of different variations, as a necessary condition for the production of divergent races and species, he has pointed out one process by which segregation is produced in nature. This one process is geographical or local separation under different environments. It may be the result of migration, or of geological and other changes in the environment; but, in either case, there is the preservation of different variations through diversity of natural selection due to the difference in the environments, and the independent propagation of the same variations due to their geographical or local separation. We have in this process an important cause of segregation resulting in divergent evolution; but no one can maintain that this is the only cause producing segregation and divergence, unless he ignores the fact that, in some cases, the isolated portions of a species, while exposed to the same environment, acquire divergent habits in the use of the environment, producing diversity of natural selection; and that, in other cases, without exposure to different environments, the very process producing the isolation, brings together those of one kind preventing them from crossing with those of other kinds, as when individuals of a special color prefer to pair together. In the former cases, indiscriminate separation is transformed into Segregation; and, in the latter cases, the isolation is segregative from the first; while, in both classes of cases, the divergence is without exposure to different environments.

Osaka, Japan.

* In "Nature," vol. xxxiv, page 407, Mr. Francis Darwin states that in his copy of Belt's "Naturalist in Nicaragua" the words "No, No," are penciled in his father's handwriting on the margin, opposite the sentence: "All the individuals might vary in some one direction, but they could not split up into distinct species whilst they occupied the same area and interbred without difficulty." This seems to give a decisive answer concerning Darwin's opinion on this subject.

ART. IV.—*The Devonian System of North and South Devonshire;* by H. S. WILLIAMS.

[Read at Toronto, Aug. 30, 1889, before Section E, American Association for Advancement of Science.]

LAST fall I had the pleasure of examining the typical sections of Devonian rocks in Devonshire, England. I went over the southern sections at Torquay, Saltern Cove and Newton Abbot and neighborhood under the guidance of Mr. Ussher of the Geological Survey, who has recently made careful study of the localities for the survey map, and in North Devonshire I had the guidance of Mr. T. M. Hall, a local geologist at Pilton, who has an admirable collection of the fossils and is personally familiar with the ground. I went across the section from Barnstaple to Ilfracombe collecting fossils myself at the fossiliferous zones of Ilfracombe, Sloly and Pilton, Toporchard, Strand and Barnstaple. In Pilton, also, I saw the admirable collection of Mr. Hall In London I examined the collections in the Jermyn Street and South Kensington Museums, at the former place Mr. Newton kindly showing me survey material not yet reported upon, and at South Kensington Mr. Etheridge showing me his original maps and pointing out the peculiarities of the sections. I also had opportunity, through the kindness of Mr. Whidborne, of examining the collection of fossils from the limestone of Lummaton. This collection, which was reported upon by Mr. Davidson in his Monograph on the British Devonian fossils is, perhaps, the finest collection in England of the fossils of that zone. In this study of the English Devonian that which impressed me most vividly was, 1st, that the fossils are very closely allied to the species in the New York Devonian, although in the great majority of cases passing under different names, and, 2d, that the rocks in their appearance, composition and order are as different as two distinct systems well can be. Not only do they differ from those of New York, but the South Devonshire section is utterly unlike that of North Devonshire, quite as unlike as the Old Red sandstone farther north is from either.

In North Devonshire, the whole series, from the Foreland grits and Lynton slates up to the Pilton beds, is made up of siliceous slates, grits and occasional argillaceous slates, and here and there intercalated beds of impure limestone. The colors are grays and purples with light browns and yellows for the grits. Slaty structure prevails throughout and obliterates or makes difficult of detection the true bedding of the rocks. Fossils are rare except in the limestone layers of the middle part and in the slates and shales of the upper part. The rocks are regarded as having a general dip a little west of south, and the outcrops lie in belts obliquely across the county from northwest to southeast and are in order, from below upward, the Lynton, Hangman, Combe Martin, Haggington, Ilfracombe, Mortehoe, Pickwell Down, Baggy, Croyde, Pilton and Barnstaple beds. The last are conformably overlaid by shales and limestones and grits of the Carboniferous age which occupy the interval separating them from the Devonian of South Devonshire. Some faults are recognized but they are few and simple.

In South Devonshire the rocks are greatly disturbed, broken by faults, standing at various angles, folded and distorted; eruptive rocks frequently cut through them and beds of volcanic ash are interstratified with them. Hence has arisen great dispute and uncertainty as to the true order of succession of the deposits although their fossils were referred to the Devonian age nearly fifty years ago.

The most conspicuous member of the Southern Devonian is the great Devon limestone. This is seen at Torquay, at Newton Abbot and farther south at Plymouth. It is blue or white in color, and sometimes red and shaly near its base. There are also great masses of argillaceous red shales which are considered as belonging above it. The character of the succession is generally interpreted to be a series of red slates and shales and grits followed by the limestone, which is again followed by a red shale. At Torquay the shore for a mile south of the town is made up of Triassic conglomerate with pebbles of Devonian limestone, fine red argillaceous slates, not only laminated but so twisted and contorted that neither lamination nor original bedding can be followed continuously for more than a few feet. With this are associated other red slates and limestones and dikes of eruptive rock. These are all so confused that except for the fossils found in them their order of sequence could hardly be determined.

One peculiar feature of the sections as seen about Ogwell and Newton Abbott is the presence of beds regularly interstratified with the limestones and shales, composed of volcanic ash. These "Schalsteins," Mr. Ussher tells me, sometimes contain fossils, and in places they are ten or twenty feet thick.

It will be seen, without comment, that the South Devonshire sections, from which most of the middle and lower Devonian fossils have been obtained, are valueless for determining the order of sequence of the faunas. There are many places (I saw such near Newton Abbott) where limestones, appearing very similar in color and structure, and within a stone's throw of each other, hold distinct faunas. In fact the interpretation of the order of the beds is a matter of the greatest difficulty even when occasional fossils appear.

In North Devonshire the difficulty is not so great, but even there the fossils are rare, and the contorted and slaty structure of the beds, even without the occasional faults, has caused grave dispute as to the true relation of the beds. As classified by Mr. Robert Etheridge, the North Devonshire section is divided into Lower, Middle and Upper Devonian, as follows :

Lower.—The *Foreland grits* and the *Lynton* and *Woodsbay slates*, with fossils in the Lynton slates and in limestone beds intercalated in the higher part of the series.

Middle.—Shales and slates with occasional thin beds of impure limestone, from the *Hangman* and *Trentishoe grits* inclusive of all the coast rocks of the north shore as far as to Morthoe. The Lower beds of Combe Martin and Haggington in their calcareous layers holding the *Stringocephalus*, or Middle Devonian fauna, and the beds about Ilfracombe hold a somewhat higher fauna.

Upper.—From the *Pickwell Down Sandstones* all the slates, shales and grits with occasional calcareous streaks across to the south line of North Devonshire at Barnstaple Bay (called *Marwood*, *Baggy*, *Croyde*, *Pilton*, *Barnstaple*, etc., beds).

It is with this section more particularly that the classification of the New York system was compared in the first surveys. And when we notice that the fauna we now consider as Lower Devonian (i. e., that of the Corniferous limestones), is little represented there and that the Hamilton fauna is largely made up of species quite distinct from those of North or South Devonshire, it is not surprising that T. A. Conrad in the preliminary reports of the State survey drew the line between the Silurian and Devonian of the New York system at the top of the Hamilton formation. Before becoming acquainted with them I expected the original Devonshire sections and fossils to throw some light upon the problem of proper classifications of the Devonian system, but I came away convinced that for internal evidence as to the order of sequence or even the precise composition of the faunas the Devonshire sections are extremely unsatisfactory. It is probable that the fossils of the Plymouth and Newton Bushel limestone formed the basis for the notion of a Middle Devonian. These limestones furnished the fossils which were recognized by Lonsdale, in 1839, as constituting a fauna intermediate between the Silurian and Carboniferous, a determination which resulted in the establishment of a new system, the Devonian. It is, however, quite uncertain what fauna followed or what preceded this limestone in South Devonshire. The rocks about Pilton and Barnstaple, Marwood and Sloly in North Devonshire furnished the originals of the Upper Devonian fauna. The Ilfracombe fauna was identified as in general equivalent to that of the South Devonshire limestones and thus became Middle Devonian, while the Lynton fauna and the faunas of the shales of South Devonshire were assigned to the Lower Devonian because they were below the Middle limestone fauna.

There appears to be no well defined Lower Devonian fauna for England nor any uniform character of deposits to represent it. What occurs below the Middle Devonian limestone is in all cases fragmental; arenaceous slates, grits, or what the Germans call "*schists*," prevail. In North Devonshire the limestone occurs in lenticular masses in the Ilfracombe slates. In South Devonshire the limestones of Plymouth, Torquay and Newton are more or less massive. In Belgium and North France they are represented by the Givetienne limestone. In the German area it is the Eifelien Kalk and the Stringocephalus limestone. In Russia, the Urals, and in Siberia a limestone holding a similar fauna is seen. Below these limestones are slates, conglomerates, sandstones, called by various names: Lynton, Spiriferen sandstein, Gédinnien, Coblenzien, etc., in many sections not recognized at all; in the more northern and western districts called "lower old red."

In North Russia, also in Wales, reaching into Somersetshire (the county bordering North Devonshire on the east) are seen the "old red sandstones." These are more typically represented in Scotland and across the Channel in Ireland.

During the same geological interval, while the estuary, or as Professor Geikie calls them, fresh-water lake deposits of the Old Red Sandstone were being deposited in the north and as far south as the borders of Somersetshire, sands and muds with occasional layers of limestone and marine fossils were deposited in North Devonshire and, still farther, in the Southern Devonshire district, and in the northern parts of Europe a limestone was forming continuously for all the central part of the period and a rich coral and purely marine fauna occupied the region. M. Charles Barrois advances the opinion that the different Devonian limestones of Europe ought to be referred to different Devonian stages, viz: the coral reefs of Erbray to the Gedinnien, those of Brittany and Spain to the Coblenzien, those of Cabrieres to the Eifelien, and those of the Ardenne to the Givetienne and Frasnienne.—*Fauna du Calcaire d'Erbray*, p. 335, Lille, 1889.

Much of the difficulty and confusion seen in attempts to correlate the various sections comes, I am convinced, from a commonly accepted assumption that formations must be correlated entire, whereas, as in the above example, the lenticular limestones of Ilfracombe undoubtedly represent the massive limestone formation of the south, while the shales and fragmental layers represent the condition of a more northern area, and doubtless when fossils are obtainable will be found to represent distinct (at least local) faunas.

It must be evident that the marine invertebrate faunas of the whole Devonian are but the equivalents of the vertebrate fauna and the flora of the Old Red Sandstone. This striking law is easier to accept and practically understand than the other, viz: that marine invertebrate faunas of very different species with few and possibly in many cases no common forms actually co-existed in the same ocean at the same time. With this second law in mind it is clear to see that the shifting of currents in the ocean—the oscillation upward and downward of the land in relation to the sea-level, the many changes in the relations of land and sea, of which there are unmistakable evidence—all these events must have produced mixing and changing of the faunas over any particular spot, not only constantly but to an extent we can scarcely conjecture from the very slight evidences preserved.

As the off-shore and deeper sea faunas of to-day differ from those living between tides on the coast, so we must believe they differed in the Devonian age. As the fauna off the Florida coast differs from that of Labrador now, so we must believe there were striking differences between the faunas of the warm ocean of the equatorial regions and the faunas of the colder polar regions of the same sea for a time as far back as such differences in climate existed.

When, therefore, we attempt to draw parallel lines to connect the stratigraphical series of New York or other parts of America, with those of England or the continent of Europe, the mere identity of species in their numerical relations is an unsatisfactory guide.

The species that are found identical both sides the sea are likely to be species whose vertical range is as long as a whole system, and the closely related forms may be either (*a*), one the successor of the other, or (*b*), one the modified migrant of the other. As to the relationship between two separate faunas, the one following the other, the difference in species and genera is often greater than that between either of the faunas and the one next below it in a like kind of deposit. As an example, we may cite our Genesee shale and Marcellus shale whose respective faunas more closely resemble each other than either of them does that of the Hamilton fauna between. To eliminate errors of this kind we should compare faunas of like deposits with each other, not because the terrane of the one region has in its beginning and ending any necessary relationship with the corresponding one of the other region, but because conditions of life are likely to have been more nearly uniform where the deposits are alike.

If we examine the Devonian limestone fauna of Devonshire we find its generic combinations very similar to those of the Corniferous limestone of our Appalachian basin, but the same genera are also seen in the calcareous strata of the Hamilton formation, and the specific types of the genera running through the whole system are more closely allied with those of our Hamilton, and even Chemung horizons than with those of our Lower Devonian. The great prominence of corals reminds us of the Corniferous, but when we compare the Brachiopods we find numerous forms, the representatives of which do not appear in our Appalachian sections till after the Hamilton terrane is passed. Such are *Spirifera disjuncta*, *Rhynchonella pugnus* and *acuminata*, *Spirifera curvata*, *Orthis striatula*, *Rhynchonella cuboides*, etc.

Some of these are not in the main limestone on the continent, and there are some indications of a separation of the fauna, in the sections of the Hartz and of Russia, more nearly corresponding to our division into Middle and Upper Devonian. And even in the more western sections of Europe the Frasnienne, as distinguished from the Givetienne limestone contains a decidedly later fauna than the latter. It appears probable that the limestones of South Devonshire represent the general interval between the close of our Corniferous and the early part of our Chemung formation.

Another problem is here suggested, viz: was there any migration of the faunas? For the determination of this point I have made a study of the Cuboides fauna, tracing it from New York to England, Belgium, France, Germany, Russia, Siberia, Persia and China. There seems to be good evidence that this fauna whose place is at the top of the Devonian limestone periods of these regions had a center of distribution nearer north France than either eastern America on the one hand or China on the other. If we take this as a uniform horizon, homotaxially, it may be said that a considerable number of species (including forms, under different names, which are very closely allied modifications of the same races) appeared before the "Cuboides" stage in the English, European and Russian sections, but not till after that stage in the New York sections.

In the sections of the interior of North America this particular fauna has not been recognized. *Rhynchonella castanea* Meek, of the MacKenzie River basin and Nevada, although in some respects resembling *R. cuboides* Sow., appears to be specifically extinct. The few species which occur in the "Cuboides zone" of Europe and range across the continent of America, in their generic history appear to represent a Carboniferous stage of development, and while abundant in the European sections are not common with us. The *acuminata* and *pugnus* types of *Rhynchonella* are known to us at the base of the eastern Chemung terrane, in Iowa at a doubtfully determined horizon, and in other western localities in association with Carboniferous faunas. In England and Europe they are conspicuous in association with what are called "Middle Devonian" faunas. *Spirifera disjuncta* is with them a Middle, as well as Upper, Devonian form. With us it is characteristic of the final Upper Devonian fauna alone.

A comparison of the fauna of the Upper Devonian of North Devonshire with our Devonian fossils shows that it is represented by our Chemung fauna, and although there are indications that it is a later fauna, as in its *Productus* with a row of strong spines along the center ("*Prod. curtinotus*" T. M. Hall), the ordinary forms are "*Productus* (or *Strophalosia*) *productoides*" and "*Chonetes Hardrensis*." The Carboniferous aspect of this Pilton fauna is not more marked than the Upper Devonian aspect of the fauna of the limestones of Ilfracombe and Newton.

Comparison of these European Devonian sections and their fossils with the corresponding ones of the Appalachian basin leads me to the hypothesis that the marine faunas of the Devonian had different histories in the two areas. There is a continuity in the succession from the lowest to the highest faunas of the system in Europe which we do not find in the American series. The explanation which seems to me most probable is that the Middle and Upper Devonian faunas of Europe (probably also down to the Lower Devonian fauna) were merely successive stages of the life inhabitants of a common and more or less continuous basin. That during this period the Appalachian basin was bounded on the east by a considerable barrier and was partially separated from the central continental basin by the Cincinnati uplift.

Up to the close of the Hamilton the Devonian faunas in the Appalachian and in the Central North American basin were extensions of the same general fauna, but they differed markedly from the corresponding European faunas.

With the Tully limestone an incursion of species of the European fauna began, and the following Chemung fauna shows a resemblance to the Upper Devonian of Europe, especially in those species which were present, themselves or in their ancestral representatives, in the European Middle Devonian. In the sections along the central part of the Appalachian basin where the Tully limestone appears holding the European "Cuboides" fauna, the Hamilton fauna is abruptly stopped, but on the eastern side of the basin the Hamilton appears to continue on, even mingling with the few

Chemung species which appear on that side, while on the western side of the basin a distinct fauna, the Waverly, succeeds the Hamilton with no trace of the Chemung or "Cuboides" faunas between.

This American Carboniferous (which we call "Sub-carboniferous"), marine fauna offers as strong contrast with the homotaxial fauna of Europe as do the respective Middle Devonian faunas of the two regions.

It seems to me not unreasonable to assume that the opening of some channel to the north or east allowed migrants of the Devonshire "Cuboides" and Upper Devonian faunas to enter the Appalachian basin, but that they did not advance far enough southward to appear west of the Cincinnati axis. While the general rise of temperature with the approach of the Carboniferous conditions caused the northward shifting of the rich "Sub carboniferous" faunas to occupy the Appalachian basin and, at the same time, the elevation of land to the northeast cut off communications from that direction and prevented any marine forms from thriving north of Pennsylvania during the period extending from the cessation of the Chemung fauna onward. From this stage on, all along the eastern and northern part of the Appalachian basin, there was no pure marine life, the sediments pass from fine red and gray muds to micaceous shales and sandstones and conglomerates, and finally, elevation of the continent into dry land is clearly indicated by the presence of coal deposits from Pennsylvania to Kansas.

Ithaca, N. Y., Aug., 1889.

ART. V.—*The Zinciferous Clays of Southwest Missouri and a Theory as to the growth of the Calamine of that section;* by W. H. SEAMON.

IN connection with the deposits of calamine in Southwest Missouri, there occur quite abundantly certain clays of peculiar physical properties and remarkable chemical composition. Their probable commercial value has been up to this time wholly unsuspected and they are thrown into the dump. The Geological Report of Missouri for 1873–74 states that 33·94 per cent zinc oxide was found in a single specimen of a reddish yellow clay found at the Fraizier diggings, Granby. With this exception the writer has been unable to learn of any previous examination of these clays and therefore hopes that this article, aside from interesting mineralogists, will lead practical miners and metallurgists to give them a trial as possible ores of zinc.

The miners distinguish between "tallow clays" and "joint clays," both of which occur associated and sometimes intermixed in every calamine digging in southwest Missouri, as has been verified by personal examination and the reports of mining men. The "joint clays" are always red in color and are also tougher and harsher in feel than the "tallow clays."

The "tallow clays" are found in layers of from several inches thickness up to two and three feet, or in lumps weighing from 50 to 500 lbs. above, below and intermixed with the crystallized and massive calamine. When taken from the ground they are generally flesh-colored, or light red and brown. On drying in air the light-colored varieties usually darken becoming various shades of brown, while the dark-colored varieties lighten becoming yellowish and sometimes ash gray. Thin streaks of a pure white variety unchanged in color on air-drying are also found in small amounts, which are characterized by a high content of zinc oxide as shown by analyses 1, 2, 3 and 4, appended.

The "tallow clays" have a peculiar greasy feel; are very fine grained and perfectly plastic; on air-drying they shrink and crumble into small fragments having a hardness of 1·25 to 1·5, which the miners call slacking. On moistening the air-dried specimens they regain their original plasticity and in a measure their original color. The air-dried specimens give off water in the closed tube; fuse on charcoal at about 3, always lightening up in color, becoming white or ash gray; give the zinc coatings when heated with soda; and are completely decomposed with gelatinization when gently heated with moderately concentrated hydrochloric acid. Their average composition is shown by analyses Nos. 5–20. In addition to these complete analyses, we have made determinations of zinc oxide in 20–25 other specimens in which the least amount of zinc oxide obtained was 21·93 per cent and the highest 39·31 per cent. The "joint clays" are usually found nearer the surface than the "tallow clays," though sometimes in close proximity to the massive calamine, filling up the crevices in the latter. While they are plastic they are tougher and not so fine grained as the "tallow clays." They are red in color, darken and shrink but little on drying. They contain zinc oxide in amounts varying from 1½ to 14 per cent., the complete analyses 20,21 and 22 appended are believed to represent their average composition. They resemble the "tallow clays" in their behavior before the blowpipe, but are not always completely decomposed with hydrochloric acid.

Analyses of "Tallow" and "Joint" Clays.

(Specimens thoroughly air-dried.)

No.	Locality.	Color as taken from the ground.	Color after drying in the air.	S. G.	H_2O at 100° C.	Loss at low red heat, H_2O mainly.	ZnO	SiO_2	Al_2O_3	Fe_2O_3	CaO	Na_2O+K_2O		Totals.
1.	Aurora, Mo.	White	White	2·91	4·03	3·92	54·06	35 29	1·64	none	1·80	none	----------	100·74
2.	" "	"	"	2·92	4·14	4·00	54·92	35·31	1·71	"	0·12	"	----------	100·20
3.	Near Peirce City, Mo.	"	"	2·95	3·63	3·52	56 12	34·82	1·52	"	0·32	undet.	----------	99 92
4.	Granby, Mo.	Gray	"	2·89	4·37	4·13	50·35	36·82	1·85	0·01	1·93	traces	----------	99·46
5.	Aurora Mo.	Flesh colored	Light drab	2·77	6·33	8·93	35·63	38 26	6 17	4·67	tr.	undet	No P_2O_5	99·99
6.	" "	" "	" "	2·78	6·53	8·73	36·16	36·90	6 29	4·22	1·02	"	"	99 84
7.	Near Peirce City, Mo.	Cream	Yellowish	---	18·06		35·64	33·36	11·03	0·80	und.	"	trace P_2O_5	99 89
8.	Aurora, Mo.	Light brown	Ash gray	2 47	9·38	9 22	36·38	36·59	4·92	1·89	1·77	none	CO_2 trace NoP_2O_5	100·14
9.	" "	Yellowish brown	" "	---	10·50	8·49	42·93	33·86	2·14	0·78	1·07	"	$NoCO_2$	99·77
10.	" "	" "	" "	2·57	9·62	8·36	30·04	37·34	10·62	2·06	1·36	"	"	99 40
11.	" "	Brown	Chocolate	2·99	7 00	10·38	28·56	43·49	5·16	4 38	1·21	"	"	100·17
12.	Granby, Mo.	---------------	Reddish brown	---	12·50	8·02	36·98	31·94	3·05	4·46	2·31	0·810	"	100·07
13.	Near Peirce City, Mo.	Brown	Pinkish yellow	---	10·44	8·19	34 33	34·21	7·91	4·89	0 02	none	"	99·99
14.	Aurora, Mo.	·	Chocolate	2·41	9·50	10.02	31 72	39·45	6·44	2·08	1·48	"	"	100·69
15.	" "	---------------	Reddish brown	2·72	6·76	9·70	32·35	37·11	3·44	9·51	1·06	traces	"	99·95
16.	" "	---------------	Brown	2·69	10 49	8·93	32·72	36·11	6·26	4·21	1·61	"	P_2O_5 . 020	100·34
17.	Near Peirce City, Mo.	Reddish brown	Yellow	2·25	10.78	9·93	34 40	37·66	3·88	3·36	0·01	none	No P_2O_5	100·02
18.	Granby, Mo.	Red	Reddish brown	---	21·58		34·78	30·27	8 78	3·98	0·08	traces	traces of CO_2 & P_2O_5	99·47
19.	Near Peirce City, Mo.	"	Pinkish	---	20·15		25·96	34·94	9 92	8 53	tr.	"	P_2O_5 . 23	99·73
20.	Aurora, Mo.	Dark red	Dark brown	---	2·48	13·61	4·30	66 22	4·91	8·44	none	none	none	99·99
21.	Phelps Co., Mo.	" "	" "	---	3·49	12·72	5·17	64 97	3·81	10·11	tr.	"	"	100·27
22.	Near Peirce City, Mo.	" "	" "	2·64	9·26	10·10	12·52	61 78	2·15	5 51	none	-----	----------	101·31

Tests for lead and cadmium were made but none found.

Thin sections of the tallow clays were examined with the microscope without yielding any results worthy of note, beyond that they appeared to be perfectly homogeneous, though no general chemical formulæ can be assigned them. They are no doubt mixtures of zinc silicate with clay, formed by precipitation from the reaction of zinc sulphide with hot siliceous waters.

The distribution of blende and calamine in Southwest Missouri shows some interesting points for study. At Cartersville, Webb City and Joplin, blende is the only ore of zinc mined. No "tallow clays" nor calamine is found. It is however true that at Webb City some of the miners call a grayish clay by the name "tallow clay," but it is wholly unlike the other and is improperly named. At Granby, calamine is the principal ore, while at Aurora, a new mining camp, both blende and calamine are found; blende being mined in one shaft, while not far distant there will be other shafts from which calamine is the only output. Lead sulphide is generally found associated with the calamine here. At Aurora, as well as elsewhere in southwest Missouri, "tallow" and "joint" clays are never found in connection with blende deposits.

The following partial section copied from the Missouri Geological Reports for '73 and '74, shows the general relations of the "tallow" and "joint" clays to the calamine.

3 feet red plastic clay.
4 inches calamine with tallow clay.
8 inches black sand and rotten dolomite.
18 inches calamine with much dolomite.
5 inches solid calamine.
6 inches red clay.

Many other sections might be quoted, which, while differing in details only, would show the same general relations of the clays with the calamine. The red clay in the above represents "joint clay." These clays frequently contain crystals of galena, and in the interior of several masses of "tallow clay" I have found in very small pockets crystals of calamine. From the relations in position of the clays and calamines it seems very probable that all the massive calamine once existed in Southwest Missouri as "tallow clays," precipitated from solutions. By filtration of waters the zinc silicate has been and is still gradually being removed from the "tallow clays" and crystallized as calamine. As the zinc silicate is removed from the "tallow clays" they pass to the stage called "joint clays," which differ from the former only in composition.

In conclusion, I desire to call attention to the fact that a clay similar in composition to the "tallow clays" has been reported from Spain (see Dana's Syst. of Min., p. 408), and that Professor Dunnington, of the University of Virginia, discovered a similar clay with an ore of zinc from the Bertha Zinc Mines, Virginia, of which the following analysis was published in No. 1144 of the Chemical News (1881).

SiO_2	37·38
Al_2O_3	24·67
Fe_2O_3	6·34
ZnO	12·10
MgO	·27
K_2O	·47
Na_2O	·27
H_2O at 100°	6·69
H_2O at red heat	10·35
	98·54

Missouri School of Mines, Rolla, Mo., Oct. 15, 1889.

Art. VI.—*On Minium from Leadville*; by J. Dawson Hawkins.

In 1885, Mr. A. Chanute, while in Leadville, collected some specimens of minium from the "Rock Mine." The specimens lay untouched in his collection until some weeks ago, when he gave me some of the mineral for examination.

The mineral was found between two ledges of outcropping rock, one of porphyry and the other of limestone, the ore of the mine being carbonate of lead, with occasional occurrences of galenite. The minium does not occur as a solid mass, but is interspersed with cerussite, and close examination also showed small particles of galenite occurring with the cerussite. The galenite found in the analysis, however, is not this. The sample taken for analysis was a very carefully picked one, a lump of the mineral being broken up, and the red particles of minium alone being taken. This sample was again carefully picked over in order to insure the absence of anything else than the pure mineral. The analysis gave the following results:

Insoluble in HCl	7·51
Pb calculated as Pb_3O_4	91·39
Fe_2O_3	·80
V_2O_5	·52
	100·22

Insoluble.	
SiO_2	2·00
$Al_2O_3Fe_2O_3$	·41
CaO	·28
Pb 4·42 PbS	5·08
	7·77

Physical characters.—Specific gravity (in powder), 4·55, 4·59; hardness, 2·5; fusibility, 1·; luster, dull; color, bright red; streak, orange-red; fracture, cubical.

From the cubical fracture of the minium, resembling that of galenite, and the occurrence of galenite in the red minium, it would appear that the minium here is a pseudomorph after galenite. The vanadic oxide which was found in the mineral, no doubt existed as vanadinite, which has been frequently found in Leadville.

The occurrence of sulphide of lead in the pure mineral is rather remarkable and also suggestive. Externally the particles of minium showed no evidence of the presence of any other mineral; it was not until the powdered mineral had been treated with hydrochloric acid and all Pb_3O_4 dissolved, that the galenite could be observed. This seems to be conclusive evidence that the minium in this case, was a direct alteration from galenite. A like deduction is forced, as regards the plattnerite lately found in the Cœur d'Alene Mts., Idaho, where all the lead ore is sulphide.

Laboratory of the Globe Smelting and Refining Co.

ART. VII.—*Mineralogical Notes*; by WILLIAM P. BLAKE.

1. *Thenardite, Mirabilite, Glauberite, Halite and associates, of the Verde Valley, Arizona Territory.*

THE deposits of sulphate of soda of the valley of the Verde river near the military post of Camp Verde have long been known, and extensively quarried, by the rancheros of the region as a substitute for salt for cattle and horses. The occurrence of thenardite in Arizona was first made known to science by the late Prof. B. Silliman, in 1881,* but he had not visited the locality and it has not been described. A recent visit to the place, and a somewhat hurried and superficial examination, enabled me, however, to collect and identify other allied species in association with the thenardite, and a peculiar pseudomorph of carbonate of lime after glauberite.

The deposits of the thenardite and the associated minerals are of considerable magnitude, covering several acres in extent, and reach a thickness of some fifty or sixty feet or more. They appear as a series of rounded hills with sides covered with a snow-white efflorescence and a greenish-colored and yellow clay at the bottom and top, partially covering the saline beds from view.

* This Jour., xxii, 204, 1881.

These beds are doubtless remnants of a much more extended deposit which occupied a local lake-like depression, or basin, probably at the close of the great volcanic era during which most of the mountain valleys of central Arizona were filled up by sediments and then overlaid by successive streams of lava. Sedimentary beds of volcanic origin remain throughout the Verde valley and its chief tributaries, and in the region of Camp Verde are deeply eroded, but rest on the uneven floor of ancient pre-Silurian slates standing on edge. High above the deposits of the valley, vertical cliffs of hard lava mark the edges of extended mesas of *malpais*, under which all the other formations are hidden and protected. But the excavations in the banks of the sulphate of soda are insignificant in comparison with the magnitude of the beds, and have failed to show, conclusively, any bottom or top, or to reveal the true relations of the beds to the surrounding formations. Whether or not they are members of the volcanic series or of a later and more local origin is yet uncertain.

Thenardite.—This salt constitutes the bulk of the deposits. It is a coarsely crystalline mass, so compact and firm that it can be broken out only by drilling and blasting with powder. It varies in its purity. Some portions are more or less contaminated with a greenish colored clay, but it is obtained also in large masses nearly colorless and transparent, with a slight yellowish tint, but seldom showing crystalline forms.

Mirabilite.—The hydrous sulphate of soda occurs in close association with the thenardite and appears to penetrate its mass in veins, but may prove to be an overlying bed. It is this species which, by its rapid efflorescence when exposed to the air, covers the whole deposit with a white powder and a thick crust through which the quarrymen must cut before they reach the solid banks of the anhydrous sulphate.

Halite.—Rock salt in beautifully transparent masses is sparingly disseminated in portions of the great beds. These crystalline masses, so far as observed, do not exceed an inch or two in thickness and no evidence of the existence of any separate workable beds could be seen. It is irregularly disseminated in the sulphate. Some masses exhibit beautiful blue tints of color, like those seen in the salt of the Tyrol and of Stassfurt. Good fragments for optical and thermal experiments could be obtained here.

Glauberite.—This anhydrous sulphate of lime and soda is an interesting associate of the other species. It occurs chiefly near what appears to be the base of the deposits, in a compact green clay. It is in clear, transparent, colorless crystals, generally in thin rhombs, lozenge shaped, with the plane angles of 80° and 100°, and from half an inch to an inch or more broad and one-eighth to one-quarter of an inch in thickness. The prismatic planes *I*, *I*, are generally nearly obliterated, or are absent, through the great development of the hemi octahedral planes -1, replacing the obtuse terminal edges. The terminal plane, *O*, is chiefly developed and this with the broad planes replacing the obtuse edges gives to some of the crystals the appearance of rhombohedrons of the minus series. The general habit of the crystals is similar to those from Westeregeln near Stassfurt described by Zepharovich;* with the predominating pyramid -1, occur also the pyramids $-\frac{1}{3}$, $-\frac{1}{2}$ and either $-\frac{3}{4}$ or $-\frac{4}{5}$; traces of a pyramid on the acute edges have also been noted. There is evidence that the crystals vary greatly in size and in their habit in different parts of the deposits. They occur also in the midst of portions of the solid thenardite as inclusions, and in one instance a small crystal was found in the midst of a transparent mass of halite. Close inspection of the transparent tabular crystals from the green clay reveals the presence of crystalline cavities with fluid inclusions made evident by the movement of small bubbles. When heated the decrepitation is violent.

Carbonate of lime pseudomorphs.—Where the lower bed containing the bulk of the glauberite crops out at the surface and has become oxidized and dried, the glauberite disappears and is replaced by carbonate of lime in an amorphous condition but having the exact form of the glauberite crystals, whose matrix they have filled. These pseudomorphs are firm, compact and dense, but are without cleavage or interior crystalline structure. Color, cream-yellow. They weather out in great numbers and show that the glauberite must occur in a great variety of sizes and forms of aggregation, in some places in rosettes and in others in crystals two or three inches long.

2. *Bournonite in Arizona.*

Bournonite occurs sparingly at the Boggs Mine, Big Bug District, Yavapai County, Arizona Territory, associated with pyrite, zinc blende, galenite and copper pyrites. The crystals are brilliant and characteristic, with interesting modifications not yet studied and compared. This is believed to be the first announcement of the occurrence of this species in the United States. I am indebted to Fred. E. Murray, Esq., superintendent of the mine, for specimens.

* Sitzungsber. Akad. Wien, vol. lxix, 1874.

ART. VIII.—*On the Spectrum of ζ Ursæ Majoris;** by EDWARD C. PICKERING.

IN the Third Annual Report of the Henry Draper Memorial, attention is called to the fact that the K line in the spectrum of ζ Ursæ Majoris occasionally appears double. The spectrum of this star has been photographed at the Harvard College Observatory on seventy nights and a careful study of the results has been made by Miss A. C. Maury, a niece of Dr. Draper. The K line is clearly seen to be double in the photographs taken on March 29, 1887, on May 17, 1889 and on August 27 and 28, 1889. On many other dates the line appeared hazy, as if the components were slightly separated, while at other times the line appears to be well defined and single. An examination of all the plates leads to the belief that the line is double at intervals of 52 days, beginning March 27, 1887, and that for several days before and after these dates it presents a hazy appearance. The doubling of the line was predicted for October 18, 1889, but only partially verified. The line appeared hazy or slightly widened on several plates but was not certainly doubled. The star was however low and only three prisms could be used, while the usual number was four. The predicted times at which the line should be again double are on December 9, 1889 and on January 30, 1890. The hydrogen lines of ζ Ursæ Majoris are so broad that it is difficult to decide whether they are also separated into two or not. They appear, however, to be broader when the K line is double than when it is single. The other lines in the spectrum are much fainter, and although well shown when the K line is clearly defined, are seen with difficulty when it is hazy. Several of them are certainly double when the K line is double. Measures of these plates gave a mean separation of 0·246 millionths of a millimeter for a line whose wave-length is 448·1, when the separation of the K line, whose wave-length is 393·7, was 0·199. The only satisfactory explanation of this phenomenon as yet proposed is that the brighter component of this star is itself a double star having components nearly equal in brightness and too close to have been separated as yet visually. Also that the time of revolution of the system is 104 days. When one component is approaching the earth all the lines in its spectrum will be moved toward the blue end, while all the lines in the spectrum of the other component will be moved by an equal amount in the opposite direction if their masses are equal. Each line will thus be separated into two. When the motion

* Read at the Philadelphia meeting of the Nat. Acad. of Sciences, Nov. 13, 1889.

becomes perpendicular to the line of sight the spectral lines recover their true wave-length and become single. An idea of the actual dimensions of the system may be derived from the measures given above. The relative velocity as derived from the K line will be 0·199 divided by its wave-length 393·7 and multiplied by the velocity of light 186,000, which is equal to 94 miles a second. A similar calculation for the line whose wave-length is 448·1 gives 102 miles per second. Since the plates were probably not taken at the exact time of maximum velocity these values should be somewhat increased. We may however assume this velocity to be about one hundred miles per second. If the orbit is circular and its plane passes through the sun, the distance traveled by one component of the star regarding the other as fixed would be 900 million miles, and the distance apart of the two components would be 143 million miles, or about that of Mars and the sun. The combined mass would be about forty times that of the sun to give the required period. In other words, if two stars each having a mass twenty times that of the sun revolved around each other at a distance equal to that of the sun and Mars, the observed phenomenon of the periodic doubling of the lines would occur. If the orbit was inclined to the line of sight its dimensions and the corresponding masses would be increased. An ellipticity of the orbit would be indicated by variations in the amount of the separation of the lines, which will be considered hereafter. The angular distance between the components is probably too small to be detected by direct observation. The greatest separation may be about 1·5 times the annual parallax. Some other stars indicate a similar peculiarity of spectrum, but in no case is this as yet established.

ADDENDUM, Dec. 17.—The predicted doubling of the lines of ζ Ursæ Majoris on December 8th was confirmed on that day by each of three photographs. Two more stars have been found showing a similar periodicity : β Aurigæ and *b* Ophiuchi (H. P. 1100 and 2909).

Harvard College Observatory, Cambridge, U. S., Nov. 12, 1889.

ART. IX—*Contributions to Mineralogy, No.* 46; by F. A. GENTH.

On a new occurrence of Corundum, in Patrick Co., Va.

IN the fall of 1888, Mr. W. B. Rucker, of Stuart, Patrick County, Va., discovered a highly interesting occurrence of corundum, and kindly communicated the following details of his find and the locality, and presented me with a box of specimens for examination, consisting of corundum with its associated rocks and minerals, andalusite, cyanite, chloritoid, mica, etc., all of which were collected on the surface of not over six acres of ground on the side of a hill, 1,800 to 1,900 feet above the level of the sea. Its continuation leads to a mountain about 3,000 feet in height and is the outlying knob of Bull Mountain, being connected with the latter by a high ridge. Bull Mountain runs parallel with the Blue Ridge and is about as high. The knob mentioned above is between 1½ to 2 miles from Stuart.

The rocks of Bull Mountain are mostly mica schists, so-called talco-mica schists, chloritic schists and slates, resembling roofing slate (not over two miles from Stuart). Some of the talco-mica schists near the corundum resemble gneiss and are highly garnetiferous and in places contain crystals of magnetite; then again, on a ridge near by, they are full of crystals of staurolite. *No serpentine or chrysolite rocks* have been observed in connection with the corundum. These rocks are intersected by several granite dikes and the corundum and its associated minerals are found on the surface, generally between the outcrops of the dikes, and probably belong to them.

Corundum.—Only a small quantity of corundum has been found; the largest piece which I have seen is 25^{mm} in diameter. All the crystals and crystalline masses appear to be *remnants* of the alteration of larger masses, into other minerals. Some of the crystals are hexagonal prisms, or, tapering at the ends, perhaps very acute pyramids with the basal plane; mostly they cluster together and form rounded masses, much intermixed with mica; on breaking they show the characteristic striation and, occasionally, are asteriated. Their color is mostly deep blue, sometimes intermixed with white, grayish and brownish white. Sometimes only microscopic grains are left, disseminated in the materials, resulting from the alteration of the corundum, viz: andalusite, cyanite, mica and chloritoid.

Andalusite.—The alteration of corundum into andalusite has never been observed before. Only a very small portion of the andalusite still exists unaltered. The andalusite crystals very closely resemble those from Lisenz in Tyrol, but only a few pieces show the common prism and basal plane. Color grayish and reddish white to flesh red. The best crystal is nearly 40^{mm} long and 20^{mm} thick and is coated with a thin film of muscovite; other masses, the largest about 80^{mm} in length, are largely mixed with muscovite and cyanite, and, occasionally, enclose some quartz.

The analyses of the purest, carefully picked out with the aid of a good lens have been made, of the grayish white variety by me (1) and of the reddish white by Mr. James S. de Benneville (2 and 3):

	1.	2.	3.
Spec. Grav.	3·154	3·151	
Loss by ignition	1·80	1·97	2·42
SiO_2	36·98	36·36	36·22
Al_2O_3	60·50	61·00	60·76
Fe_2O_3		0·72	0·88
MgO	0·10		
Corundum	1·12	trace	trace
	100·50	100·05	100·28

Cyanite and Rhætizite.—Both the typical blue, bladed cyanite and the so-called rhætizite occur pseudomorphous after andalusite, some of the specimens indicating that the latter has occurred in stout crystals, the largest from 70 to 80^{mm} in size.

The blades of the cyanite are of a bluish white to sky-blue color and often from 10 to 25^{mm} broad, in many specimens, however, much smaller, sometimes radiating and gradually becoming masses of interwoven fibers. Associated are small quantities of quartz and muscovite which latter especially lines the cavities. Blue corundum in small grains is disseminated through the mass.

The rhætizite of a grayish brown color and a more or less fibrous structure is the more frequent form of alteration of the andalusite, and, in breaking the masses, many show in the interior the prismatic forms of the original andalusite. It is often intermixed with a large quantity of grains of blue corundum, muscovite and rarely of chloritoid.

Muscovite.—There is hardly a specimen of the andalusite, rhætizite and cyanite in which muscovite could not be observed as a direct alteration of these minerals. In the rhætizite it is frequently found in somewhat larger quantity and, together with chloritoid, often with a nucleus of blue corundum. This muscovite has a brownish white color. A partial analysis of it gave:

Loss by ignition	6·49
Na_2O	0·87
K_2O	9·23

Margarite (in part).—No crystallized variety of margarite has been found, but some of the andalusite, still retaining the original form, has been altered into a soft, fine-grained, or compact mineral, in some portions discolored by ferric hydrate, and mixed with some fine scales which are probably muscovite. After purification with dilute hydrochloric acid, it was analyzed by Mr. Jas. S. de Benneville (*a*) and me (*b*) with the following results:

	a.	*b.*
Loss by ignition	5·56	5.40
SiO_2	33·38	35.79
Al_2O_3	46·49	45.95
Fe_2O_3	1·43	1·03
CaO	6·02	5·49
Na_2O	2·47	2.27
K_2O	2·33	2·82
Corundum	1.70	2·07
	99·38	100·82

The analyses indicate a mixture of several micas, margarite predominating.

Chloritoid.—This mineral which has been so frequently observed with corundum, as at Mramorskoi in the Ural, at Gumugh Dagh in Asia Minor and elsewhere, is also found with the corundum of Bull Mountain. In the bladed cyanite it is found in small quantity, but in the rhætizite, associated with muscovite it surrounds a nucleus of blue corundum, from which it originated.

The foliated masses have a blackish green color, the largest is 30^{mm} in diameter. The sp. gr. I found = 3·614. My analyses gave:

	1.	2.
Loss by ignition	6·64	6·58
SiO_2	25·03	25·53
Al_2O_3	39·75	39·23
FeO	22·92	----
MnO	1·30	1·14
MgO	3·32	3·32
CaO	0·21	----
Na_2O	0.07	----
K_2O	0·07	
	99·31	

The pure mineral contains no ferric oxide.

A short distance from the locality where these minerals have been found, indications of the occurrence of the same species have been observed in several places.

This occurrence of corundum is entirely different from any previously described.

Some specimens of blue corundum in grayish brown rhætizite, discovered several years ago by Mr. J. A. D. Stephenson, of Statesville, N. C., at Hunting Creek, north of Statesville are very similar; there was also a mass, consisting of crystals of andalusite, altered into a micaceous mineral which, however, was not further investigated.

Chemical Laboratory,
111 S. 10th St., Philadelphia, November 16, 1889.

ART. X.—*Origin of Normal Faults;* by T. MELLARD READE, C.E., F.G.S., etc.

IN the October number of this Journal, Professor LeConte gives an explanation of the origin of normal faults marked by his well known lucidity and graphic power.

The conception is however not a new one, as may be seen on referring to Geikie's Text book of Geology (page 315, first ed.), Beete Jukes' Manual of Geology, and a paper by I. M. Wilson and correspondence thereon in the Geological Magazine for 1868.

As I happen to have devoted a good deal of time and study to a consideration of the same subject, I may perhaps be allowed a little criticism. In the first place I would point out that Professor LeConte has left out of consideration the expansion of the strata above by the heat of the intumescent, molten mass required to produce anticlinal tension and which would completely neutralize such tension excepting at or near the surface. As an illustration, I may point to the fact that normal faulting was not produced by the intumescent masses or laccolites which gave birth to the Henry Mountains. What occurred was radial splitting, not parallel faulting and the slipping down or lifting up of several blocks. It may be urged that this is not a parallel case as the basin faults resulted from an intumescence on a much grander scale with less proportional arching or doming up. It is demonstrable that the heat of such an enormous molten mass would, by expansion, put the superincumbent strata into compression, excepting near the surface, for the lengthening of the strata by increase of temperature would far exceed the lengthening by arching. Any tension would be confined to the surface layers and the rifting would be radial.

Again there is another difficulty connected with the "flotation theory." It is not yet explained how, if the underlying molten matter is of greater specific gravity than the material of the crust, it can ever well over the surface in sheets of lava. I do not say this objection cannot be surmounted, but it leaves the theory very incomplete until an explanation has been given.

A complete or satisfactory theory of normal faulting should account for the phenomena all over the world, and not merely in the basin region. I venture to affirm that there is no evidence other than the requirements of certain theories of the earth, of such a universal, or nearly universal sheet or zone of molten matter as is assumed by Professor LeConte. It is also reasonable to ask if the crust of the earth be an extremely thin shell, floating upon a sub-crust liquid, whence comes the lateral pressure required for the formation of mountains by folding?

It has been shown by myself * and by Davison, Darwin and Fisher, that the secular cooling of the earth cannot, on the most favorable assumptions, bring the outer shell of the globe into compression below a depth variously estimated at from two to five miles, the compression being greatest at the surface and diminishing to zero within these figures of depth or to what has been termed the "level of no strain." The introduction of a molten zone between a thin upper crust and a solid nucleus will not help matters in any way.

Normal faults, as the name implies, are not confined to the Basin region; they are world wide. They frequently occur in extensive areas where there are no volcanic rocks, dikes, sheets, ash beds or other signs of "relief of tension by outpouring of lava or by escape of stream." As associated with strata of mountain ranges, they are more frequent in the surrounding plains than in the folded portions exhibiting signs of great compression.

The theory of normal faulting favored by Professor LeConte is inapplicable outside of the Basin region, and local explanations not in harmony with general phenomena are to be suspected.

In my view, normal faulting is not the result of tension through intumescent upheaval, *but of decrease of bulk of the underlying matter of the crust itself.* This decrease may result from the cooling and consolidation of injected igneous sheets, or from the cooling of portions of the solid heated matter of the crust, or from both combined. In whatever way a diminution of bulk occurs, the overlying strata will by gravity follow up the shrinkage and preserve the solidity of the crust.

This, I have attempted to show, must, through mechanical necessity, take place by shearing and wedging up of the sheared blocks. It has frequently been observed by geologists that in faulted regions the strata seem as if they had been drawn apart, and that if replaced in their original positions they would not fill the void. Contraction of the strata explains this fact, but tension by arching does not.

It would take up too much of the space of this Journal to go into further details; but any one who may desire to pursue the subject, will find the theory fully set out in Chapter VIII of the Origin of Mountain Ranges, and further discussed in my paper on slickensides and normal faults in the Proceedings of the Liverpool Geological Society 1888–9; also in a series of articles entitled Theories of Mountain Formation in "Research" for 1888.

Park Corners, Blundellsands, in Liverpool, England.

* Origin of Mountain Ranges, Chap. XI, and various papers in the Philosophical Magazine and Philosophical Transactions by the authors named.

ART. XI.—*On the Estimation of the Optical Angle by Observations in Parallel Light;** by ALFRED C. LANE.

§ 1. WHENEVER in a rock section we are lucky enough to find a mineral showing a bisectrix squarely in convergent light, and large enough to give a definite image with Bertrand's lens, we can employ his method as given by Rosenbusch, Lévy et Lacroix, etc.† Occasionally the method developed by Michel-Lévy,‡—of noting the angle that must be turned to bring the image from that of a cross to that of two hyperbolas tangent to a given circle,—may be used here. Supplemental to these methods or in sections in which they cannot be used, nearly or quite as good values of 2V (*not* 2E or H) may be obtained without finding β, by comparing the order of colors in different sections between × nicols. These colors are dependent on the different retardations of the wave fronts in passing through the crystal, and in petrographical textbooks (Rosenbusch–Iddings, Michel–Lévy et Lacroix) are tables giving the relative retardation of one wave front, corresponding to different Newton's colors, for a unit thickness. This difference is proportional to the double refraction.

$$\text{i. e. (the order of color)} \sim o-e \sim \delta\left(\frac{1}{u_o}-\frac{1}{u_e}\right) \qquad (1)$$

Where o and e are the thickness of air that would be traversed by the wave fronts in the time that they actually take to traverse the crystal plate, u_o and u_e are the two wave fronts (or beam) velocities, and $\frac{1}{u_o}$ and $\frac{1}{u_e}$ the indices of refraction for the given section. δ is the section thickness.

But for the optical angle we have§

$$u_e^2-u_o^2=a^2-c^2 \sin\theta \sin\theta' \qquad (2)$$

Where a b c are proportional to the three principal velocities and inversely to α, β and γ, least, middle and greatest indices of refraction, (by a slip of the pen in Dana's Textbook, p. 147, both a and α are called maximum,) θ and θ' are the angles to the principal optical axes from the normal to the wave fronts. Hence they involve 2V.

* From the unpublished report of the Michigan Geological Survey, published by permission of M. E. Wadsworth, State Geologist.

† See also E. Mallard, Cristallographie Physique, vol. ii, p. 416.

‡ Lévy et Lacroix, p. 94.

§ Lloyd, Wave theory of Light, §§ 231, 232, 216.

Then if we let $\frac{a+c}{u_e+u_o} \cdot \frac{u_e u_o}{a c}=1$ (see Mallard, p. 116) which will make less than 1 per cent error we have from 1 and 2,

$$o-e=\delta(\gamma-\alpha)\sin\theta\sin\theta' \qquad (3)$$

§ 2. In the rhombic system we must search for pinacoidal sections. The brightest of these is parallel to γ and α. Which of the other two is ‖ to α and β the determination of axial plane and sign will settle. Then we may derive from (3),

$$(o-e)_{\gamma\alpha}=\delta(\gamma-\alpha) \qquad (4)$$
$$(o-e)_{\alpha\beta}=\delta(\gamma-\alpha)\sin^2 V \qquad (5)$$
$$(o-e)_{\beta\gamma}=\delta(\gamma-\alpha)\cos^2 V \qquad (6)$$
$$\tan^2 V=\frac{(o-e)_{\alpha\beta}}{(o-e)_{\beta\gamma}} \qquad (7)$$
$$(o-e)_{\alpha\beta}+(o-e)_{\beta\gamma}=(o-e)_{\gamma\alpha} \qquad (8)$$

Equation (7) is the best to find V, but if the observations are adjusted to satisfy (8), which serves as a check upon our accuracy, either (5) or (6) may be used.

For example we may pick out the required sections in hypersthene by some of the following marks: 1) ‖$\gamma\alpha$ one cleavage direction, ext. $+o$, brightest polarization colors, pleochroism green to red brown, prismatic form with flat dome, no axial image. 2) ‖$\gamma\beta$ only one cleavage direction, ext. $+o$, lowest polarization colors of any such section, pleochroism yellowish-brown to green, direct emergence of bisectrix. 3) ‖ $\alpha\beta$, two cleavages nearly at right angles, diagonal ext., quadratic forms, pleochroism yellowish-brown to reddish-brown. If we meet, in a mineral of any crystallographic system, sections which prove by use of convergent light to be principal sections, we may of course use them. This often happens in mica.

§ 3. In monoclinic minerals we handle but two cases: (a) the elongation is parallel to β:b. Then *if a large enough number of cases be taken,*

$$\frac{\text{No. of cases in which the elongation is} +}{\text{No. of cases in which the elongation is} -}=\frac{-2V}{+2V}$$

But practically in applying this to the commonest case, epidote, we find that in many rocks the elongation is almost always—. This is because the crystals are really tabular through large sized (100). I note in passing, that contrary to Rosenbusch's experience, I find the orthopinacoid (100) the most important face of epidote, one easily recognized too, by the yellow color of c nearly perpendicular to it, and frequently by the twinning.

b). The elongation lies in the axial plane $\alpha\gamma$ (hornblende augite, etc.) In this case sections directly across or parallel to the elongation must be used.

Let two sections, both perpendicular to the plane of symmetry, and respectively parallel and perpendicular to the elongation, have indices of refraction $(\gamma'\beta)$ and $(\alpha'\beta)$ respectively. Let φ be the angle from c to the prismatic axis and let V also be measured from c. Then θ and θ' will become $(\varphi+90°-V)$, and $(\varphi+90°+V)$ or $(\varphi-V)$ and $(\varphi+V)$ respectively. Then substituting in (3) we may transform it into the following forms: $2V'$ is the value of $2V$ that would be obtained by treating $\alpha'\beta$ and $\gamma'\beta$ as principal sections.

$$\cos 2V = 2.\frac{\delta(\alpha'-\beta)}{\delta(\gamma-\alpha)} + \cos 2\varphi \qquad (10)$$

$$= 2.\frac{\delta(\gamma'-\beta)}{\delta(\gamma-\alpha)} - \cos 2\varphi \qquad (11)$$

$$= \frac{\delta(\gamma'-\beta)+\delta(\alpha'-\beta)}{\delta(\gamma'-\beta)-\delta(\alpha'-\beta)}\cos 2\varphi = \cos 2V' \cos 2\varphi \qquad (12)$$

$$\delta(\gamma-\alpha)\cos 2\varphi = +\delta(\gamma'-\beta) - \delta(\alpha'-\beta) \qquad (13)$$

In a slide of even thickness δ is constant and cancels out. $(\gamma-\alpha)$ and φ will be determined in the same slide. Eq. (13) being an equation of condition controls the observations.

The character of $(\gamma'-\beta)$ and $(\alpha'-\beta)$ whether + or − must be determined by mica plate or otherwise. Eq. (12) is the best one to use. It is easy to make a diagram for its graphical solution. See figure. Throwing (12) into the $\left(\frac{\gamma'-\beta}{\alpha'-\beta}-1\right)\left(\frac{\cos 2V}{\cos 2\varphi}-1\right)=2$, we see that we may consider $\frac{\gamma'-\beta}{\alpha'-\beta}$ the ordinate y. $\frac{\cos 2V}{\cos 2\varphi}$ the abscissa x of a rectangular hyperbola whose asymptotes are $x=+1$ and $y=+1$ and intercepts on ox and oy are $(\bar{1},o)$ and $(o,\bar{1})$. This hyperbola once constructed we can find $\frac{\cos 2V}{\cos 2\varphi}$ for any value of $\gamma'-\beta$ and $\alpha'-\beta$ at once. Then will any point on the line through (o,o) and $\left(\frac{\cos 2V}{\cos 2\varphi}-1\right)$ have its abscissa proportional to 2V if its ordinates is proportional to $-\cos 2\varphi$. Hence by drawing a set of lines diverging from O and by marking the values of φ against the corresponding lengths of $-\cos 2\varphi$ on ON and the values of v against the lengths of $\cos 2V$ on the same scale (i. e. to the same radius) on NP we can read off V for a given φ. (See example below). We need only to construct the hyperbola for one quadrant if we remember that x for y is equal to $-x$ for $\frac{1}{y}$, but we must be careful about the signs.

§ 4. Formula (12) etc. may be applied to the hornblendes by seeking out three kinds of sections: 1) Prismatic, with only one cleavage direction, with maximum extinction of the prism zone, *with highest polarization* colors, and often with characteristic pleochroism. (These give $\delta(\gamma-\alpha)$). 2) Prismatic, with one cleavage direction, extinction *o* with lowest colors of the prism zone, and characteristic pleochroism (gives $\delta(\gamma'-\beta)$. 3) Across the prism, with two *equally developed* cleavages at an angle of about 56°, + extinction bisecting the acute angle, and characteristic pleochroism. These give $\delta(\alpha'-\beta)$. When twinning exists it also helps in selection.

In estimating the order of an interference color between × nicols, if the natural color interferes, it often is better to raise and lower the color a wave length or so and take the mean of the orders thus obtained. This may be done with a mica or gypsum plate, for it is hardly worth while to use the compensator.

To illustrate the application of equation 12. In 11670,* a hornblende schist, we have a hornblende with pleochroism, *c* sea-green, *b* brownish-green, *a* yellow and sections whose colors indicate the following double refractions:

$\delta(\alpha'-\beta,$	$\gamma'-\beta,$	$\gamma-\alpha)$	$c:c$	Remarks.
−300	+125	+400	{ 16— / 13	Twin, $\delta(\gamma-\alpha)$ for epidote runs up to 1400
300	175			" " orthoclase " 275
350	150			
300	175	350	{ 13 / 16	Twin, " " apatite " 125
260				
		420 to 520 }	{ 19 / 15 tr	Twin, varying shades, the lighter color the greater $\gamma-\alpha$
		420 / 680 }	{ 15 / 19	Twin, " "
——	——			applying eq. 13.
−303	155	——	——	
mean	−303	+420+ maximum	170	420(cos 34°=0·829)=348 $<$ 458
	——			By eq. 12
	458			$\cos 2V = -\frac{155}{458} \cdot 829 = -0{\cdot}28 = \cos 106°$
				$\therefore -2V = 74°$

Of course the required sections are never exactly found; we use the nearest approach to them we have. In the example above the unsymmetrical extinction shows that there is an error in $\gamma-\alpha$. Now while any error in the $\alpha'\beta$ and $\gamma'\beta$ sections is about as likely to give us higher colors as lower, so that the mean of different observations should be taken (if δ is constant), errors in determining $\gamma-\alpha$ will make it too small. Thus the largest value should be chosen and even then it will be too small. Eq. 13 will show the discrepancy, which serves to measure our error. As we have the equation of condition (13), a connection between $\gamma-\alpha$ and 2φ, different degrees of liability to error in finding the given

* The numbers refer to sections of rocks belonging to the collection of the Michigan Geological Survey.

sections, and a varying angle to measure, it would be hard to say what weight would attach to the probable error as generally computed. I have, however, picked out a number of slides containing the common hornblende of the amphibolites for which 2V has been determined. In five slides I have noted the number of sections used in determining 2V and weighted the results accordingly. Twelve other slides have been combined without weighting. Each set gives $-2V=79°$ with probable error in one case of $0°\cdot 11$, in the other of $1°\cdot 08'$. The possible error is certainly much larger.

Deducing $\gamma-\alpha$ by comparison, and from that and 2V, $\gamma-\beta$ and $\beta-\alpha$ (this may also be done graphically by the figure), we find by these methods in the epidiorites and hornblende schists a series often intergrown, thus:

	$\parallel c$	$\parallel b$	$\parallel a$	$\gamma-\alpha$	$\alpha-\beta$	$\gamma-\beta$	$-2V$
1. Actinolite	slightly bluish	colorless	colorless	0·026 decreases always	0·013 increases sometimes	0·013 decreases always	88°
to	to dull	to	to	to		to	to
2. Common hornblende and on toward glaucophane	bluish-green to !	olive green to :	yellow :	0.019	0·012	0·007 and keeps on?	79° and lower

The relations of increase and decrease may be seen very clearly in cases of intergrowth. The series is not a linear one, but we leave the full discussion* for the report of the Michigan Geological Survey.

EXPLANATION OF THE FIGURE.

This figure is to illustrate the graphical determination of 2V from the double refractions—or vice versa—as above referred to. It is founded on the formulæ

$$\left(\frac{\cos 2V}{\cos 2\phi}-1\right)\left(\frac{\alpha'-\beta}{\gamma'-\beta}-1\right)=2=(x-1)(y-1)$$

$$\cos 2V-2\left(\frac{\alpha'-\beta}{\gamma-\alpha}\right)=1-1-\cos 2\phi=x-2y$$

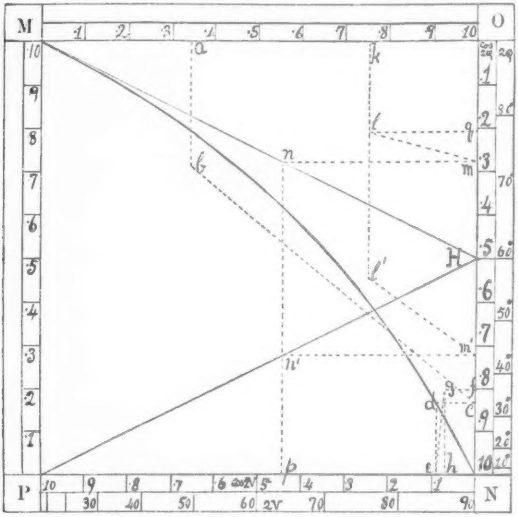
$$-\cos 2V-2\left(-\frac{\gamma'-\beta}{\gamma-\alpha}\right)=1-1-\cos 2\phi=x-2y$$

where 2V = optical angle about $c=\frac{1}{\gamma}$, α' and γ' are at right angles to each other and to β, and $\phi=\angle^{\gamma'}\gamma$.

* The change seems mainly due to imbibition of $Na_2Al_2Si_4O_8$, but this requires chemical proof. If this is so, however, in view of this rapid falling off in γ, continued in glaucophane, which contains about 65 per cent $Na_2Al_2Si_4O_8$, and of the—extinction of the undetermined glaucophane mineral, described by Rosenbusch (Mikr. Phys. ii, 319), it seems likely that the pure $Na_2Al_2Si_4O_8$ amphibole has $c=\alpha$. [Between writing this and reading proof, the description of it as Riebeckite, essentially $Na_2Al_2Si_4O_8$, has been given.] We have analogous relations in acmite and ægirine.

It may be shown that by exchanging $\alpha'-\beta$ and $-(\gamma'-\beta)$ we shall only exchange cos 2V and −cos 2V. So that we need consider but the following cases:

1. In the rhombic system $\phi = o$ to find 2V, (a) given $\delta(\alpha-\beta)$ and $\delta(\gamma-\beta)$ with $\gamma-\beta > \beta-\alpha$. Lay off along MO proportional to $\delta(\gamma-\beta)$ the abscissa Ma, and the ordinate ab in the same proportion to $\delta(\alpha-\beta)$. From the point b proceed in the direction mb till you strike ON at c. From c proceed ‖ to NP, meeting the hyperbole at d, then turn at right angles and go ‖ON till we meet NP at e. Then eN = + cos 2V and 2V corresponding may be read from the outer scale. If $\gamma-\beta < \beta-\alpha$ we must interchange and 2V will be −.



(b) Given $\delta(\gamma-\alpha)$ and $\delta(\alpha-\beta)$ to find 2V. Lay off from M an abscissa Mk proportional to $\delta(\gamma-\alpha)$ and as an ordinate kl in the same proportion to $\delta(\alpha-\beta)$. From l proceed in the direction Ml till you strike ON, at m. Thence go ‖ to OM till we strike the zigzag MHP, at n. Turning at right angles go ‖ to ON till we strike NP, at p. pN (positive) is + cos 2V if n is on MH, −cos 2V if n is on HP. Interchange for $\delta(\gamma-\alpha)$ and $\delta(\gamma-\beta)$.

2. In the monoclinic system, ϕ being given, and $\delta(\alpha'-\beta)$ (a) and also $\delta(\gamma'-\beta)$ greater than it. Proceed as in 1ª till we get e. Thence go toward O till we meet a line ‖ to OM, and at a distance fO = cos 2ϕ from it, at g. From g drop upon NP again, at h. hn is + cos 2V if cos 2ϕ is +, otherwise −. Interchange as usual.

(b) And also $\delta(\gamma-\alpha)$. Find pn as in 1ᵇ and subtract 1 − cos 2ϕ, which may be read off from ON from it.

3. Given 2V and one of the three $(\gamma-\alpha)$ $(\gamma-\beta)$ $(\gamma-\alpha)$; to find the other two work backward; e. g. (a) given $\gamma-\alpha$. If pN = cos 2V erect a perpendicular cutting PHM at n' and n. Draw lines through n and n' ‖ to MO, cutting ON at m and m'. Draw mM and m'M. Lay off Mk = $\gamma-\alpha$ and construct an ordinate, cutting mM and m'M at l and l'. Then kl and kl' are $\alpha-\beta$ and $-(\gamma-\beta)$ [if cos 2V is +].

(b) $(\alpha-\beta)$ given. Proceed as before 3ª till we get m and m'. Then lay off on ON Oq = $\alpha-\beta$. Erect a perpendicular ql to ON, cutting mM at l. Then draw the line kll' at right angles to gl, Mk is $\gamma-\alpha$ and $l'k = \gamma-\beta$. Of course these various coördinate lines should be drawn once for all in complete sets.

Michigan Mining School, Houghton, Mich., Sept. 14, 1889.

ART. XII.—*A new Stone Meteorite;* by L. G. EAKINS.

THE meteorite which forms the subject of this paper was brought to notice by Prof. R. T. Hill, of the University of Texas, who presented the piece first obtained by him to the National Museum. This piece, of an irregular shape, and weighing about two and a half kilograms, is supposed to be but a fragment of a much larger mass, which Prof. Hill expects to obtain.

It has a superficial coating of a yellowish brown color where it has been subject to weathering, but on a fractured, unaltered surface it is dull black with a slight grayish tinge. It is hard, compact, and very tough; to the unaided eye the stony mass is very uniform in structure, and none of the composing silicates can be distinguished, but troilite can be plainly seen scattered throughout it, and on a ground and polished surface the metallic particles are also visible. Under the microscope, (for which information I am indebted to Mr. Whitman Cross,) the stony portion seems to consist chiefly of olivine and enstatite, with a small quantity of a colorless mineral which is probably a feldspar; as will be seen later, the analytical results also indicate the presence of a feldspar, while chromite also was found unmistakably.

The mass has a specific gravity of 3·543 at 30°, and its analysis as a whole is as follows:

SiO_2	44·75
Al_2O_3	2·72
Cr_2O_3	·52
Cu	tr.
FeO	16·04
Fe	1·83
NiO	·52
Ni	·22
Co	·01
MnO	tr.
CaO	2·23
MgO	27·93
K_2O	·13
Na_2O	1·13
P_2O_5	·41
S	1·83
H_2O	·84
	101·11
Less O for S	·92
	100·19

From some of the finely powdered material the metallic portion was extracted by the aid of an electromagnet, and as slight amounts of troilite and the silicates remained attached, this was then treated with a neutral solution of copper sulphate and the solution analyzed. This gave the metallic part as constituting 2·23 per cent of the mass, with the following composition, calculated to 100 per cent:

Fe	88·74
Ni	10·68
Co	·58
	100·00

The residue from which the metallic portion had been removed was then digested with dilute hydrochloric acid; this dissolved the troilite and olivine, and the soluble and insoluble portions were then separated and analyzed in the usual manner. The sulphur present was calculated as troilite, with the formula FeS, and has been deducted in stating the analysis of the soluble portion:

	Soluble in HCl. (Troilite deducted.)			Insoluble in HCl.		
	Analysis.	Calculated to 100%.	Molecular ratios.	Analysis.	Calculated to 100%.	Molecular ratios.
SiO_2	15·67	38·13	·636	30·36	56·14	·936
Al_2O_3	1·06	2·58	·025	2·02	3·73	·036
Cr_2O_3				·54	1·00	·007
FeO	8·12	19·76	·274	4·95	9·15	·127
NiO	·49	1·19	·016			
CaO	·42	1·02	·018	1·94	3·59	·064
MgO	15·34	37·32	·933	13·22	24·44	·611
K_2O	undet.			·10	·19	·002
Na_2O	undet.			·95	1·76	·028
	41·10	100·00		54·08	100·00	

In the soluble portion the ratio of the RO group to the SiO_2 is as close to that of olivine as could be expected in work of this kind. The insoluble portion, after removing the Cr_2O_3 and a proportional amount of FeO to form chromite, gives these ratios:

$$\begin{array}{ccccc} RO & : & Al_2O_3 & : & SiO_2 \\ \cdot825 & : & \cdot036 & : & \cdot936, \end{array}$$

which seem to bear no definite relations to each other. It is noticeable, however, that the Al_2O_3 and alkalies present are nearly in the proportion required for a feldspar, and if one of the oligoclase type were present in proportion to this Al_2O_3, it would have these molecular values:

$$\begin{array}{ccccc} RO & : & Al_2O_3 & : & 4\tfrac{1}{2}\,SiO_2 \\ \cdot036 & : & \cdot036 & : & \cdot162; \end{array}$$

assuming this to be the case, and deducting this amount of feldspar it would leave

$$\begin{array}{ccc} RO & : & SiO_2 \\ \cdot789 & : & \cdot774, \end{array}$$

which corresponds closely to enstatite.

By calculating the soluble and insoluble parts as having been determined on the original material minus the metallic portion and water, and taking all the sulphur as representing troilite (FeS), the general composition of the meteorite is shown to be

Metallic	2·23
Troilite	5·03
Soluble in acids	39·84
Insoluble in acids	52·42
	99·52

Laboratory U. S. Geological Survey, Washington, D. C., October, 1889.

ART. XIII.—*On the Barium Sulphate from Perkins' Mill, Templeton, Province of Quebec;* by EDWARD S. DANA.

SOME six months since M. A. Lacroix* described, under the name of *Michel-levyte*, a mineral having the same composition as barite, $BaSO_4$, but as he believed crystallizing in the monoclinic system. The discovery of the dimorphism of barium sulphate is a point of so great mineralogical and chemical interest that it seems necessary to scrutinize closely the grounds upon which the conclusion rests.

The mineral from Perkins' Mill is described as occurring in masses showing three cleavages, two of them inclined to one another at angles of 78° and 102°, and a third at right angles to these—the geometrical form then, so far as known, is that of normal barite. The cleavages, however, differ in kind: one of those first named was found to be highly perfect, the surfaces having a marked pearly luster; this is taken by Lacroix as the orthopinacoid, h^1 (100). The cleavage corresponding in position to the other prismatic face of barite was difficult; this is taken as the base, *p* (001). The remaining cleavage, corresponding to the base of barite, was intermediate between the other two in character and gave surfaces having a vitreous lus-

* Comptes Rendus, cviii, 1126.

ter; this is made the clinopinacoid, g^1 (010). The measured angles were $ph^1 = 102°$ to $103°$, $pg^1 = h^1g^1 = 90°$. Further a polysynthetic twinning is described, resembling the triclinic feldspars, the face h^1 being the twinning plane and composition face; "macles par interpénétration" are also mentioned, but not minutely described.

Optically, one axis of elasticity is normal to g^1, this is taken as probably the bisectrix, while the axial plane makes angles of 134° and 124° with h^1 and p respectively in their obtuse angle; in other words the axes of elasticity in g^1 make angles of 5° with the diagonals of the section formed by h^1 and p. The refractive indices are given, but their values are very nearly identical with those accepted for barite. The specific gravity (4·39) and composition are those of barite.

In conclusion, therefore, we find that the monoclinic nature of the Perkins' Mill barium sulphate rests upon the following grounds: the difference in character between the two cleavages, h^1 and p; the presence of enclosed twinning lamellæ seen in sections parallel to g^1; and the deviation of the axes of elasticity of about 5° from the diagonals of the rhombic section formed by the two cleavages h^1 and p.

Through the kindness of Mr. G. Christian Hoffmann, of the Canadian Geological Survey, to whom we are indebted for many important contributions to our knowledge of Canadian minerals, the writer has received a considerable supply of specimens (upwards of fifty) of the barium sulphate from Perkins' Mill. The spot from which these were obtained corresponds in position with that described by Lacroix, and the individual, who furnished Mr. Hoffmann with the specimens, remembered having given others from the same place to a French gentleman (whose name he did not recall) in the summer of 1888.

Upon the first examination of the specimens the observer is struck with the beautiful pearly luster of one of the cleavage surfaces, corresponding to the face called h^1 (100) by Lacroix. The mineral occurs in cleavable forms only, and varies all the way from those which show cleavage surfaces of several square inches to those which are coarsely granular only. Portions of the enclosing granular crystalline limestone are often seen.

Attention was directed first to the most important variation from the requirements of the orthorhombic system reported, viz: the position of the axes of elasticity in the cleavage section, which is normal to the two oblique cleavages (prism of barite), and parallel to the face g^1 of Lacroix (base of barite). A large number of carefully selected cleavage fragments were taken, upwards of thirty from a dozen or more different specimens. These failed, however, to confirm the measurements of Lacroix.

On the contrary, the extinction direction was found to bisect the obtuse and acute angles as exactly as it could be determined; the individual measurements rarely varied more than from 30′ to 1° from this position. So far as the specimens under examination go, then, there is no variation in the position of the axes of elasticity from that required by normal barite. The relative values of the axes of elasticity were also found to conform to those of barite, and in a section cut normal to the line bisecting the interior obtuse angle (102°) of the cleavages h^1 and p the optic axes were visible, the axial plane being parallel to the shorter diagonal of the rhombic section. Optically, therefore, the specimens examined conform to normal barite.

In the nature of the cleavages the supposed monoclinic character seems to gain more support. The pearly luster of one cleavage surface (h^1 of Lacroix), parallel to which the specimens readily separate into thin plates or scales, is, as has been remarked, a striking feature of the mineral. Parallel to the cleavage which is obliquely inclined to this pearly face (p of Lacroix), the fracture takes place usually with difficulty, and the surface exposed then shows a multitude of fine lines, which are the edges of the plates parallel to h The remaining cleavage is usually, as noted by Lacroix, less difficult than that of p and the surface is generally vitreous in luster.

An examination of a large number of specimens, however, shows that these characters are not constant. Occasionally a mass is found in which there is no surface of pearly luster at all, and in which the character of the cleavage faces could not be distinguished from ordinary barite—such specimens are rare. On the other hand, we find specimens in which the pearly luster and the tendency to separate into thin plates belongs to *both* surfaces of oblique cleavage (prismatic faces of barite) so that it is impossible to distinguish between them. The pearly luster is also sometimes present over a portion of one of the surfaces and absent over the remainder; again, in some specimens this character belongs also to the third cleavage direction (g^1). Moreover, a close examination of the pearly surface shows an iridescence between the plates due obviously to their slight separation, while scales of a foreign substance can often be detected between them.

In short, the writer regards it as clear that the apparent easy cleavage (laminated structure) and the accompanying pearly luster of one of the prismatic faces are secondary in origin and have been called out by pressure—a force the action of which can easily be understood in the case of masses enclosed as these are in crystalline limestone.

Several thin sections have been examined, cut parallel to each of the three cleavages. The sections parallel to what would be the base of barite (g^1) show a nearly uniform development of the cleavage cracks parallel to the two oblique directions. Between these lines, and in *both* directions, (h^1 and p, Lacroix), though somewhat more marked in one of these, are a series of inclusions of the same mineral, having nearly uniform extinction and so orientated that one of the prismatic cleavages is very nearly parallel to the line bisecting the obtuse angle of the cleavage faces of the specimen as a whole. These parallel lines of inclusions vary much in width and on the whole are irregular in outline, though showing a tendency to take a crystalline form. They stand out sharply when the section is examined in polarized light and suggest at once something of the nature of polysynthetic twinning. A careful measurement gave the angle between the similar cleavages in adjacent portions as 56°, and, if the two are actually in twinning position, the twinning plane (referred to the barite form) might then be the macroprism 210, which would require 54° 21′, or the brachyprism 130, requiring 54° 29′. It is to be noted, however, that the extinction-directions vary a few degrees among themselves, and further, besides these inclusions with nearly regular orientation, there are many others which are totally irregular in position. Moreover, a macroscopic examination of such a cleavage surface shows long parallel lines of inclusions with cleavages in nearly the same position and yet changing a few degrees from one to the next, not only in the direction of the prismatic zone (ph^1) but also at right angles to it. This last fact sufficiently explains why in the included portions the extinction-directions vary as much as the 5° of Lacroix from the diagonals of the rhombic section of 78° and 102°.

It seems, therefore, more probable that there is no definite twinning involved in any of these cases. It should be added that these sections show what look at first like very narrow twinning lines following the cleavages, but examination shows them to be, for the most part, only the open cracks between the plates of the mineral. The section parallel to the other faces show also lines of inclusions chiefly in the direction of the prismatic edge (h^1/p), but while some of these keep the same optical orientation, most of them are irregular and many are made up of a multitude of minute grains.

Another feature of this barium sulphate is the presence upon a prismatic face, especially when it shows the pearly luster, of fine striations closely crowded together and parallel to the basal edge. This seems to be due to the development, probably by pressure, of a steep pyramidal face inclined from 5° to 8° to the prism. Furthermore, another set of similar lines upon the same face, are often seen inclined about 45° to the basal edge as if caused by the partial development of a macrodome; this set is also at times crossed by another at right angles to it, that is, inclined about 45° to the basal edge in the opposite direction. These points merit a closer study than it has been possible to give them thus far. They remind one of the twinning lamellæ parallel to the macrodome 601 described by Bauer,* but they do not seem here to be connected with any variation of optical orientation.

In conclusion, the writer regards it as proved beyond doubt for the specimens which he has had in hand from Perkins' Mill, that they are normal barite; and that the existence of a monoclinic form of barium sulphate among specimens from that locality is extremely doubtful.

SCIENTIFIC INTELLIGENCE.

I. Chemistry and Physics.

1. *On the Decomposition of Carbon disulphide by Shock.*—On attempting to reduce liquid carbon disulphide by means of a liquid alloy of potassium and sodium, THORPE obtained a yellowish-brown powder encrusting the alloy, which exploded violently when the bottle was shaken; this powder being found subsequently to detonate with more violence than diamine di-iodide. The hand of the operator was coated with a black deposit, consisting apparently of finely divided carbon. Reflection convinced the author that this deposit was far larger than could have come from the decomposition of the brown powder itself, and as carbon disulphide is an endothermic compound, absorbing in its formation according to Thomsen 19610 calories, it appeared not at all improbable that, like acetylene, cyanogen, nitric and nitrous oxides, the oxides of chlorine, etc., it might experience decomposition by a sudden shock; and that in the above case, the carbon disulphide had been resolved into its elements by the violent explosion of the yellowish brown powder. Actual trial showed that mercuric fulminate would produce the same effect; and Thorpe suggests the experiment as an easy and safe method of showing in the classroom the decomposition of an endothermic compound by shock. For this purpose a thick glass tube about 600^{mm} long and 15^{mm} wide is fitted at one end with a rubber stopper carrying two stout wires, on the lower end of one of which is a small cup, like a deflagrating spoon, while the other wire is bent so as to come within 2 or 3^{mm} of the bottom of the cup. In this cup is placed about 0·05 gram of mercuric fulminate. A piece of paper slightly longer than the tube is moistened with carbon disulphide and placed within the tube and the cork is put in. After a minute or so, the tube being inclined at 45°, the vapor of the disulphide will practically fill it and the paper may

* Jahrb. Min., i, 37, 1887.

be withdrawn. On passing now the spark from an induction coil through the fulminate by means of the wires through the stopper, it at once explodes and the walls of the tube are seen to be lined with a deposit of soot mixed with a small quantity of mercuric sulphide and sulphur. If the tube be filled with nitrogen or carbon dioxide the deposited carbon is dense, lustrous and coherent. Other explosives were ineffectual.—*J. Chem. Soc.*, lv, 220–223, May, 1889. G. F. B.

2. *Light and Electricity.*—Professor RIGHI (Riv. Sci. Ind., July–August) shows that ultra violet radiations reduce to the same potential two conductors, a plate and a piece of grating, applied to each other, the rays being thrown on the grating side. He suggests a convenient way of measuring differences of potential of contact. One notes the deflection of an electrometer connected with the plate (the grating being permanently connected with earth); then, having connected the electrometer for an instant with earth, makes the radiations act a sufficient time. He used a zinc electric lamp, and the metals examined were placed in some cases in a bell jar, to which some gas or vapor was admitted. From measurement of different plates with the same metallic net (copper, zinc, or platinum), the differences of potential of pairs of metals could be deduced. Prof. Righi found the differences sensibly the same in dry and moist air and in carbonic anhydride: but with hydrogen, very different values (from those in air) appeared, where one of the metals examined was platinum, palladium, nickel, or iron (doubtless due to absorption). In ammonia all the metals, examined with zinc net, seemed to have become less oxidizable; and in coal gas, carbon and platinum behaved like more oxidizable metals.—*Nature*, Nov. 7, 1889, p. 18. J. T.

3. *Galvanic Polarization.*—E. WARBURG has made a careful and long continued study of this subject and concludes that previous observers have not sufficiently taken into account oxidization of the metallic electrode in electrolytes containing oxygen. The metallic electrodes are surrounded thereby with a weak solution of salt of their own metals, and the electromotive force changes with the concentration of this solution.—*Ann. der Physik und Chemie*, No. 11, 1889, pp. 321–344. J. T.

4. *A Simple modification of the method of Telescope and Scale reading.*—The well known method consists in reading the reflection of a scale placed at a certain distance from a movable mirror, in this mirror, by means of a telescope. Since the telescope must be focussed on a reflection which is as far behind the mirror as the scale is in front, the range of adjustment is limited. H. E. J. G. DUBOIS has modified this method by placing a second fixed mirror in front and inclined to the movable mirror. With a telescope one can then read a division of the scale put at twice the usual distance from the movable mirror. With a telescope magnifying 18 times and with an objective of $3{\cdot}5^{cm}$ one can read to two seconds angular measure.—*Ann. der Physik und Chemie*, No. 11, 1889, p. 494. J. T.

II. Geology and Mineralogy.

1. *North American Geology and Palæontology,* for the use of Amateurs, Students and Scientists; by S. A. Miller. 664 pp. large 8vo. Cincinnati, Ohio.—All geologists and paleontologists who have had occasion to use Mr. Miller's invaluable "American Palæozoic fossils" and who thereby know of the thoroughness and accuracy of his work, will give this new volume a hearty welcome. All but the first 100 pages constitute really a new and enlarged edition of the above mentioned work, increased much in value by the introduction of numerous figures, there being scarcely a page without one or more. All Paleozoic species are included in the tables with their latest names and synonymy, and references also to places of publication.

The first part of the work is a brief review of the geological formations commencing with the oldest, giving descriptions of the rocks, their distribution, their characteristic fossils, and other features.

The author, in this part, uses the term *Taconic* of Emmons, dating, as he states, from 1842, in place of *Cambrian* for beds below the rocks of the New York series or the Postdam sandstone, and says that the word Cambrian was first proposed in England "some years after that of Silurian," 1835 being the date of the latter. He overlooks the fact that Sedgwick proposed the term Cambrian also in 1835. "Silurian" was proposed by Murchison in the Philosophical Magazine for July of that year. Then at the meeting of the British Association the next month, August, a communication on the "Silurian and Cambrian" was presented by "Professor Sedgwick and R. I. Murchison," in which each explained his own system of rocks. Sedgwick's first systematic account of the Cambrian system was published in 1838, in the Proceedings of the Geological Society of London, the same year in which, though later, Murchison's completed monograph, "The Silurian System" appeared. This was four years before Emmons's report of 1842.

Mr. Miller's work will be found of great value to geological and paleontological students and the necessary companion of all investigators of the Paleozoic rocks. J. D. D.

2. *The Geological and Natural History Survey of Minnesota for the year* 1888, the 17th Annual Report; by N. H. Winchell, State Geologist. 273 pp. 8vo, St. Paul, Minn.—This volume contains a report by Prof. Winchell on the lower rocks of Minnesota; a second, by Mr. H. V. Winchell on the work of 1888 in the Iron Regions of the State; and a third, by Mr. U. S. Grant, on work in 1888 in northeastern Minnesota.

Prof. Winchell goes over the questions relating to the Archæan and Cambrian ("Taconic") rocks, and presents his views at length with reference to their characters and arrangement. The Archæan of the state is divided into (1) the Laurentian, gneiss, (2) the Vermilion schists, and (3) the Kewatin schists, the latter two adjoining the gneiss but unconformable to it. Above these there follow: with over-lap unconformity, (1) the "*Taconic*" beds stated to be of the Olenellus horizon, and including the Animike and Huronian; (2), with over-lap unconformity, the "*Potsdam*," including quartzytes (with gabbro and red granite), referred to the Paradoxides horizon, on the ground of fossils reported from the Pipe clay district of southwestern Minnesota at the southwestern extremity of the belt; (3) the "*St. Croix*" beds, "of the Dikelocephalus horizon" with only over-lap unconformity between them and the Potsdam, and graduating above into the Calciferous magnesian limestones.

Prof. Winchell makes the beds of iron ore of the Animike to correspond in age and relation to iron ore-beds in the Taconic formation of western New England. But this formation has no such beds, the only iron ore being limonite, of secondary origin, except some local bodies of iron carbonate. Moreover the Taconic limestone, in which the limonite deposits and iron carbonate occur, has afforded in some places Calciferous or Trenton fossils. The latest discovery of this kind was made in 1889 by W. B. Dwight (this Journal, xxxviii, 150), in the Copake-Millerton-Amenia limestone-belt, in which are several of the great limonite-deposits, and at Amenia the largest body of iron-carbonate yet observed in the Taconic region; and here the fossils of the limestone were species of Ophileta, Orthoceras, Cyrtoceras.

There is a misapprehension on page 9, that should be noticed. It is there stated that a second edition of Dana's Manual of Geology was issued in 1864, two years after the first. The author of the work knows nothing of such a second edition. There was an issue of the work that year from the stereotype plates, and the publishers may have inserted 1864 on the title page; but if so, it was not a new edition. Moreover such a method of moving on the date the author has always protested against.
J. D. D.

3. *Geology of the Quicksilver Deposits of the Pacific Slope.* 486 pp. 4to, with a folio atlas of 14 plates, by GEORGE F. BECKER. Vol. xiii, Monograph of the U. S. Geological Survey.—In his report Mr. Becker treats of two subjects of prime geological importance—that of the metamorphic Cretaceous rocks of California, and the related one, the origin of the deposits of quicksilver. The former he had previously presented, but less fully and less strongly; in this new volume the facts are so clearly set forth, and are so well fortified, the gradations from non-metamorphic to metamorphic made so plain, that all doubts should disappear even from those who have been relegating all serious rock-crystallization to Archæan time. The metamorphosed rocks are proved *by fossils* to be for the most part at least Lower Cretaceous or Neocomian in age; and in constitution they were granitic sandstones and shales, containing feldspar, quartz, and more or less mica, with often hornblende. The new minerals made by the metamorphism include muscovite, augite, hornblende,

glaucophane, zoisite, saussurite, feldspars even in the half-altered sandstones, of which oligoclase, labradorite, and orthoclase are occurring kinds, the first of the three most common, the last, rare; also epidote, garnet, chlorites, serpentine, rutile, titanite, zircon, apatite. The rocks include diabase, dioryte, (for which metamorphic kinds the author proposes the term *pseudodiabase, pseudodiorite*, overlooking the terms metadiabase, metadioryte, proposed in 1876, and used in this Journal in a paper by Mr. G. W. Hawes); coarsely crystalline forms of the diabase or gabbro, with zoisitic and hornblendic varieties; glaucophane schists, containing quartz, a soda feldspar, usually some zoisite and mica, often garnets, passing into gneiss-like varieties on one side and into thin-schistose on the other, and, at Mt. Diablo exhibiting a distinct passage from shales to the schist, proving, as Mr. Becker says, that "the schistose structure is an original feature, not a result of metamorphism"; phthanyte, or flinty silicified sandstones or shales; serpentine. The serpentine is found to have been produced through an alteration of the sandstones, all the kinds having undergone the change; and the minerals altered to serpentine include augite, hornblende, feldspar, chlorite, garnet, and even quartz and apatite. Mr. Becker discusses the conditions of these and other metamorphic changes, and throws much light upon the question of origin.

From the part of the work on quicksilver deposits much might be here cited which is of prominent geological importance. The discussion of the origin of the deposits leads also to observations on the origin of metallic deposits of other kinds. The author had visited the mines of Europe before writing his report. The atlas contains geological maps of differing mining regions, and also diagrams of mine-workings, mine-sections, and other matters of economical interest.

4. *A new locality of Lower Silurian Fossils in the Limestones of Columbia Co., N. Y.;* by I. P. Bishop. (Communicated by the author).—In October, 1887, I found near Pulver's Station, about 2½ miles north of Philmont, Columbia Co., N. Y., and within territory heretofore considered as Taconic, an outcrop of limestone. A very brief search revealed unmistakable organic remains, among which were several gasteropods, crinoid stems and a cast of a single brachiopod valve. A few days later I visited the spot again and brought away several specimens, only one of which, a *Multiculopora*, could be identified. More urgent duties prevented any further search for fossils that season; but in the spring and summer of 1888, I visited the place several times and made a careful examination of about one-third of the whole outcrop. The organic remains proved to be not only more abundant than in the other fossiliferous localities previously discovered,* but to be in greater variety and in a better state of preservation. As many as six or seven Orthocerata were plainly distinguished; but owing to the massive character of the limestone, I

* Vide this Journal, vol. xxxii, p. 438, 1886.

was not able to get a specimen which could be identified. Gasteropod remains were very abundant, forming almost the whole of certain thick layers. Of corals I obtained seven or eight specimens in a fair state of perservation.

The fossils which I carried away were submitted to Professor C. E. Beecher of Yale College Museum, who identified the species:

Chaetetes compacta Billings.
Monticulopora lycoperdon Say.
Orthis testudinaria(?) Dal.
Murchisonia gracilis.
Orthoceras, Sp.

Professor Beecher says that in his judgment the specimens are from the Trenton group, probably from the lower part. I may add that the color and general appearance of the rock go to strengthen this view.

The outcrop of limestone within which the above mentioned fossils were found lies about a mile southeast of Pulver's Station, and not more than 300 yards from the Harlem railroad track. It has the same northeasterly strike as all the rock in this region, and a dip of about 50° to the southeast. The whole exposure is approximately one-third of a mile long, with an average width of 150 yards. It is especially interesting for the reason that there is no other limestone outcrop nearer than a mile, and no vestiges of fossils in any rock within more than two miles. The whole fossiliferous area is surrounded by highly metamorphic schists and slates which extend to an unknown distance on the south and east. From its fossils, its appearance, and its relation to the rocks lying to the eastward, I judge this limestone to belong to the same geological horizon as the other fossiliferous limestones previously found by me north of Chatham in the same county.

5. *Shallow-water origin of the Cincinnati shale and limestone.*—An interesting paper on this subject by Mr. N. W. Perry, in the December number of the American Naturalist, finely illustrated by phototypes, shows conclusively that, in accordance with the views of Professor Newberry and the later observations of Professor Shaler in Kentucky and Prof. J. F. James in Ohio, the Cincinnati shales and limestone are of shallow-water origin. The phototypes represent rain-marks, ripple-marks, and mud-cracks, of the most characteristic kind. Mr. Perry concludes that the rocks were made over the gradually sinking bottom of a shallow sea.

6. *The Lower Cretaceous of the San Carlos Mountains, Mexico.*—Dr. C. A. White, the author of the paper on this subject in the last volume of this Journal, states in a letter of November 19th to the editors: "I have no doubt that the 4,000 feet of limestone which I found in the San Carlos Mountains of Chihuahua, were accumulated on a subsiding sea-bottom. Deep sea forms seem to be either wanting, or very rare. I did not detect any forms from top to bottom of the series that might not have lived in comparatively shallow waters."

7. *The Sabre-toothed tiger and other Quaternary Mammals of Florida.*—Dr. JOSEPH LEIDY has named the Quaternary tiger, of which remains were received by him from Mr. Joseph Willcox, the *Drepanodon* or *Machairodus Floridanus.* The specimens are from Ocala, Marion Co. It was a somewhat smaller animal than the Brazilian species. The same locality has afforded also a præmolar of *Elephas columbi*, and the tooth of a Llama. On Pease Creek, in Florida, Mr. Willcox obtained other Mammalian remains, among which Dr. Leidy has identified *Tapirus americanus*, a *Hippotherium* or *Hipparion*, a *Glyptodon*, named by Dr. Leidy *G. septentrionalis*, and remains of a turtle, *Emys euglypha* Leidy.—*Proc. Acad. Nat. Sci. Philad.*, 1889, p. 29, 86.

8. *Fossils of the Western Taconic limestone in the eastern part of Dutchess Co., N. Y.*—A letter of October 14th, from Professor WM. B. DWIGHT states that he had found, in limestone near the Clove Valley Station, *Calciferous* fossils, including the common Fucoids, with Ophiletas, probably *O. complanata;* proving that the Fishkill belt of limestone is Lower Silurian, and consequently, in connection with his other discoveries, that all the belts of limestone of Dutchess County are fossiliferous.

9. *Cambrian fossils from the limestone of Nahant, Massachusetts*, northeast of Boston; by A. F. FŒRSTE.—The fossils discovered at this locality and reported upon in the Proceedings of the Boston Society of Natural History for 1889, p. 261, are *Hyolithes* resembling specimens found by Prof. Shaler at North Attleboro, not far from the boundary of Rhode Island. The latter species was referred to *H. princeps* Billings. Mr. Fœrste names it *H. inaequilateralis*, and makes the large Nahant specimens the same. The limestone northeast of Mill Cove, in North Weymouth, 12½ miles from Nahant, is referred to the same horizon, which is made that of the Olenellus group. The nearest Paradoxides beds are at Malden.

10. *The Development of some Silurian Brachiopoda;* by C. E. BEECHER and J. M. CLARKE. 96 pp. 4to, with 8 plates. Memoirs of the New York State Museum, Vol. I. Albany, Oct., 1889.—This memoir is the result of a careful study of a number of Brachiopods with reference to their successive differences in the progress of development. The specimens were all from a single locality in the Niagara limestone at Waldron, Illinois. The variations in the several parts with the increase in size are described in detail, and many general conclusions are reached. One of these conclusions of much interest is that, in confirmation of observations of Brooke and Morse on living Brachiopods, the shell in its early stages approaches a subcircular outline, so that there is uniformity in the embryology of the ancient Silurian types and that of the modern species. The memoir is an important study in evolution.

11. *Devonian Plants from Ohio;* by Dr. J. S. NEWBERRY.—In the Journal of the Cincinnati Society of Natural History for October last (page 48), Dr. Newberry describes and figures of

natural size the *Caulopteris antiqua* Newb., of the Corniferous limestone. The fine specimen was from Sandusky, Ohio. In the same rock occur *C. peregrina* Newb., *Sphenophyllum vetustum* Newb., and *Lepidodendron Gaspianum* Dawson. The *C. peregrina* is also figured and described, together with *Dadoxylon Newberryi* of Dawson, and the Sphenophyllum and Lepidodendron mentioned.

12. *U. S. Geological Survey Bulletins.*—The Survey has recently issued Nos. 48 to 53 of its bulletins, as follows: No. 48. On the Form and Position of the Sea-Level, by R. S. WOODWARD. 88 pp. 8vo.—No. 49, Latitudes and Longitudes of certain points in Missouri, Kansas and New Mexico, by the same.—No. 50, Formulas and Tables to facilitate the construction of Maps, by the same.—No. 51, On Invertebrate fossils from the Pacific Coast, by C. A. WHITE, 70 pp., with 14 plates.—No. 52, Subaerial decay of rocks and origin of the red color of certain formations, by I. C. RUSSELL, 60 pp. 8vo.—No. 53, The Geology of Nantucket, by Prof. N. S. SHALER, 56 pp. 1889.

13. *Geological Survey of Missouri.*—A letter from Mr. ARTHUR WINSLOW, dated Jefferson City, states that he has been appointed geologist of Missouri and has entered on his duties.

14. *Geologie der Münsterthals in Badischen Schwarzwald*, by Dr. ADOLPH SCHMIDT, Prof. Univ. Heidelberg. (Carl Winter).—The first part of this work on the geology of the Münsterthal appeared in 1886, the second in 1887, and the third, in 1889. The last extends to 112 pages, and treats of the ore-deposits; first of the associated minerals and their paragenetic combinations, and then of their paragenesis. Dr. Schmidt is one of the best authorities on the subject, and his work sheds light on mining deposits generally.

15. *Contribuzioni alla Flora Fossile dei Terreni Terziarii della Liguria;* by S. SQUINABOL. I. *Fucoidi ed Elmintoidee*, Roma, 1888; II. *Caracee-Felci*, Genova, 1889.—These two contributions seem to be the beginning of a somewhat extensive work by the author on the fossil flora of Liguria, but as the first was published in the Bolletino of the Italian Geological Society in octavo form and the second by the University of Genoa in quarto form they cannot be brought together into a volume. Judging from the work already done by Gaudin, Massalongo, Sismonda, Sordelli, and others in the beds of this age in Liguria and the adjoining provinces the greater part of the fossil plants met with are dicotyledonous leaf impressions, and it is gratifying to note that the lower forms are receiving attention. The fucoids described in the first paper are problematical organisms referred to Chondrites, Laminarites, Zonarites, Münsteria, Helminthoida, and a new genus Eoclathrus. There are twenty species, twelve of which are new. They appear to represent a formation equivalent to the Flysch of Switzerland. The second paper describes one species of Chara and thirty-two ferns belonging to eighteen genera. The figures are for the most part photo-

graphs of the specimens and too indistinct to form a judgment from. The author seems to have thoroughly ransacked the literature of the subject and his "Elenco Cronologico" which forms the second part is a very full list of papers relating in any way to the fossil botany of Italy, considerably fuller than that of Portis. It embraces 262 titles. L. F. W.

16. *Jarosite from Utah;* by F. A. GENTH. (Communicated).—Messrs. Geo. L. English & Co. have recently brought from the Mammoth Mine, Tintic District, Utah, interesting varieties of *Jarosite* in minute crystals, lining cavities of a siliceous limonite, and sometimes associated with a pulverulent, yellow mineral, probably a basic ferric sulphate. The crystals are of a yellowish brown to dark clove-brown color and a very brilliant vitreous luster; they are very small, from about 0·1 to 1^{mm} in size, and look so much like cubes with tetrahedral planes, that they were mistaken for pharmacosiderite. A closer inspection, however, showed their rhombohedral forms. Prof. Samuel L. Penfield had the kindness to examine them for me, and gives the following information. "The crystals are so rounded that they will not give distinct and satisfactory reflections. From a very small crystal I obtained $R \wedge R$ 88° 27′, while Naumann gives 88° 58′ for jarosite, an agreement as close as I could expect. I also identified the base, and a very small plane $-2R$. I was able to produce basal cleavage."

Even the best specimens placed in my hands by Messrs. English & Co. did not furnish me with absolutely pure material for analysis, owing to the fact that the crusts are very thin and the crystals stick so fast to the siliceous matrix and often enclose the latter that only at the expense of a great deal of time and patience, about one gram of nearly pure fragments of crystals could be obtained (I); analysis (II) was made with somewhat larger and darker crystals. Both show a slight contamination with siliceous limonite—but the analyses leave no doubt that the mineral is *jarosite.* Spec. grav. of I (taken in alcohol) = 3·163. The analyses gave:

	I.	II.
SiO_2	0·08	0·29
Fe_2O_3	50·41	51.16
Na_2O	9·23	0·33
K_2O		9·05
SO_3	29.60	28.93
H_2O	10·68	10·24

Chem. Laboratory, 111 S. 10th st., Philadelphia, October 13, 1889.

17. *Brief notices of some recently described Minerals.*—REDINGTONITE, KNOXVILLITE. Two hydrous chromium sulphates from the Redington mine, Knoxville district, California. They occur at a depth of 150 feet at a point where solfataric gases still issue and are regarded as the result of the solfataric action upon chromic iron. Redingtonite occurs in masses with a fine fibrous structure and of a pale purple color; the extinction is oblique (13° to 38°) and a triclinic form is suggested. A qualitative

analysis showed it to be a hydrous chromium sulphate containing some aluminum and iron.

Upon the redingtonite occurs another chromium sulphate containing less water; it appears in rhombic tables of 78° and 102° with cleavage parallel to the base, macropinacoid and prism. Isomorphism is suggested with copiapite (which, however, is monoclinic according to Linck). It is noted that redingtonite is changed by heating into this second chromium sulphate. These minerals, with also napalite, are described by Becker in Monograph XIII, (U. S G. S.) on the quicksilver deposits of the Pacific slope (see p. 68, of this number.)

NAPALITE. A mineral resin occurring with pyrite and millerite in vesicular quartz in the Phœnix mine, Mayacmas district, Napa Co., California. It has a consistency like that of shoemaker's wax; the color is dark reddish brown and it shows a green fluorescence by reflected light, which, however, disappears upon exposure to the air. The hardness is 2, the specific gravity 1·02. It is brittle, inelastic, with a conchoidal fracture; by the warmth of the hand it can be so softened as to be moulded and drawn into long threads. It becomes liquid at 46° and boils at 300°; at 130° a heavy colorless oil distils over, while a heavy dark red oil is later obtained having a boiling point about 350°. Analysis showed the composition to correspond to C_3H_4.

MESSELITE. A hydrous phosphate of calcium and iron found in a bituminous clay-slate near Messel in Hesse. It occurs in small tabular crystals, often grouped in star-shaped forms. They are colorless or pale brown, with hardness 3 to 3.5. From the optical characters they are referred to the triclinic system. An analysis gave

P_2O_5	FeO	CaO	MgO	MnO	H_2O	insol.
37·72	15·63	31·11	1·45	*tr.*	12·15	1·40=99·46

This leads to the formula $(Ca, Fe)_3P_2O_8+2\frac{1}{2}H_2O$, which brings it near fairfieldite. Described by W. Muthmann in *Zeitschr. Kryst.*, xvii, 93, 1889.

RAPHISIDERITE. A name given by A. Scacchi to minute acicular crystals of iron sesquioxide from Pianura and Fiano, which have been examined by E. Scacchi and found to have a rhombic section with an angle of about 72½°. It is not certain that they are not hematite.—*Att. Accad. Napoli*, Dec. 1, 1888.

COHENITE. A name given by Weinschenk to crystals occurring in the Magura, Arva, meteorite. They are indistinct in form but are probably to be referred to the isometric system; they are very brittle, have a tin-white color, hardness 5·5 to 6, and specific gravity 6·977. An analysis gave

Fe 39·78 Ni(Co) 3·57 C 6·65=100.

This leads to the formula $(Fe, Ni, Co)_3C$.—*Ann. Mus. Wien*, iv, 93, 1889.

WARRENITE. Mr. L. G. Eakins has informed the editors that he has given the name Warrenite, after Mr. E. R. Warren of Crested

Butte, Col., to the sulphantimonite, $3(Pb, Fe)S \cdot 2Sb_2S_3$, described by him in this Journal for Dec. 1888.

18. *Das Reich der Krystalle* für jeden Freund der Natur insbesondere für Mineraliensammler leichtfasslich dargestellt von H. BAUMHAUER. 364 pp. 8vo. Leipzig, 1889 (W. Engelmann).—Dr Baumhauer has succeeded in presenting the subject of crystallography in an unusually attractive form. The general morphological relations are given after the commonly accepted methods, but besides this the growth of crystals and crystal groups is explained and illustrated by a large number of excellent figures which reproduce nature's forms with remarkable success. The physical characters of crystals are also briefly but clearly stated, and particularly the molecular structure as developed by etching, a subject to which the author has himself made important contributions.

III. BOTANY.

1. *Die natürlichen Pflanzenfamilien*, von A. ENGLER UND K. PRANTL. [Engelmann, Leipzig.]—In the earlier numbers of this Journal we have taken occasion to speak in terms of high commendation of this work now in course of publication. Two new parts have just come to hand, and they can be praised as heartily as any of their predecessors. Part 37 is devoted to Clethraceæ, Pirolaceæ, Lennoaceæ, and a portion of Ericacæ proper. Part 38 treats of the reminder of Ericaceæ, together with Epacridaceæ and Diapensiaceæ, all by Professor Drude of Dresden, and the order Myrsinaceæ by Pax, of Breslau.

A short account of the treatment of the orders Clethraceæ and Pyrolaceæ by Professor Drude, will indicate to our readers some of the points of peculiar excellence which this work presents. But such an account will give also an opportunity of stating distinctly, once for all, what seems to us a marked defect in the whole treatise.

The treatment begins with a citation of the more important literature of the subject. Then follows a half-page illustration, giving the leaf and flowers of *Clethra arborea* and *tinifolia*, together with numerous analytical details, all of them possessing great clearness and beauty.

The ordinal characters are well described, and then the author gives a sketch of certain peculiarities presented by the organs of vegetation, considered rather from a biological point of view. Next comes a section to which we think exception must be taken, namely, the statement of the supposed relations of the anomalous genus Clethra. On the strength of a suggestion by Klotsch, the author places the genus Clethra in an order by itself, but he does not give plainly the reasons which have controlled him in this disposition of the group. The evidence which he adduces cannot certainly be all that which proved convincing to his own mind, and he should have given his readers the advantage of all the light which he himself possessed. We should not mention this, were it not for the fact that at many places through the book, the editors of the orders have made very grave changes in the orders and suborders, without giving all the evidence in the case. Many of the changes seem to us desirable, but even in these, the plainest, the statement of the new relationships is not sufficiently full for all readers, and it seldom compares favorably with the fullness in other portions of this excellent work.

Continuing our analysis of the treatment of the order, we come next to a short but clear statement of the geographical and paleontological relations of the order, and the subject closes with a synoptical view of the subgenera.

The same author treats the Pyrolas and associated plants under one order, *Pirolaceæ*. The whole treatment is substantially that just described. After the citation of authorities, and the description of the distinguishing features of the group, the writer presents an interesting account of the economy of these plants, and passes, by the way of *Pyrola aphylla*, a partial saprophyte, or humus-loving plant, to the Monotropas and their associates, all of which are true saprophytes. There is a graphic account of the structure of the roots of these plants and their attached fungi, through the intervention of which it is supposed they obtain their nourishment. The anatomical peculiarities are given with considerable detail. There is one point with reference to the roots, which should be very carefully examined in our species, namely, their extraordinary power of enabling the plant to multiply by adventitious buds formed on them. We do not ordinarily look for buds on roots.

The author speaks particularly of the coloring matter held in the tissues of these saprophytes and the effects of agents, especially alcohol, thereon. In short, here as elsewhere, the editors have left out very few facts which can interest the student, the subject of cross-fertilization and the like, receiving much attention. All the more important features throughout, which require illustration, are clearly and copiously figured. The section on geographical distribution is short and telling. It is followed by a paragraph on the supposed relationships, which to our thinking is less satisfactory even than that referred to under Clethraceæ. The reasons adduced for separating these plants from their old time allies, the other Ericaceæ, appear trivial in the extreme. But, of course, these are questions of judgment. It seems, however, as if the editor ought to have given all his reasons for this separation and presented his case in full. It seems ungracious to allude to this, but since the treatise is likely to occupy a place not filled by any other work on Botany, the most important portion of the subject of classification should have demanded rather more space for its elucidation. The treatment of the order closes with a grouping of the genera into suborders, and a description of the genera themselves. The generic descriptions are almost full enough to warrant us in saying that in most instances, it would be possible to use the work as a handy Genera Plantarum.

The illustrations and typographical execution leave nothing to be desired. G. L. G.

2. *On the effects produced on some tropical plants by a temperature of* 40 *to* 34 *degrees.**—In horticultural language, a stove is a hot-house, specially adapted for the cultivation of tropical plants. The temperature at which it is kept, varies, of course, widely, according to the season, but its range in winter is generally from 55° or 60° Fahr. at night, to 70°, Fahr. in the daytime. In a well regulated establishment, the temperature is not allowed to fall below the minimum just mentioned. If, through accident or neglect, the temperature descends below 45° but is still above freezing, certain plants soon exhibit signs of having sustained injury. In the case of some of the *Crotons*, plants cultivated for their foliage, the injury is shown in the partial withering and subsequent fall of the leaves: in the instance of *Eucharis*, a diminished quantity of flowers is the most obvious result. The temperature at which these injuries are produced may be considerably above that of the freezing point of water, and therefore the question of frost is to be left out of account. If the plants were in the open air, the question of frost might well come in, for if a plant is exposed to the open air, on a clear night, when the sheltered thermometer marks a temperature of say 45°, the radiation may carry the temperature of the plant even below 32°. In a greenhouse, this reduction of temperature by radiation is practically impossible, and can be left out of consideration. Hence we have to study only the effects produced by the temperature which is indicated by a thermometer close by the plant.

In order to approach the question as part of a larger inquiry which I have had for some years under investigation, it was thought best that the tissues of certain tropical plants should be examined microscopically, under varying conditions of heat and cold. In the present communication, the principal results of this examination between the somewhat arbitrary limits of 40° and 34° will be briefly detailed. 39° is said to be the point at which water reaches its greatest density, while 34° was considered by me to be well within the danger limit so far as frost is concerned. First, as regards the effect on the cell-wall in all the cases observed: there was no physical injury apparent. Second, as regards the effect on the protoplasmic contents: there was merely a reduction of rate of circulation. Third, there was no appreciable change in the size of the sap-cavities, (vacuoles). Fourth, there was a notable reduction of the power of plasmolytic agents, such as Potassium nitrate, solution of cane sugar, etc. This pointed plainly to a diminution in the power of absorption.

It is well known that the temperature of the soil has a marked influence on the absorption of liquids by the roots, cold diminishing the rate of absorption. Comparative experiments now in progress indicate that in this respect tropical plants are even more sensitive than subtropical, and much more so than those of temper-

* Read Nov., 1889, before American Academy.

ate climates. In the delicately balanced economy of tropical vegetation, even the slight disturbance resulting from carrying the temperature down below the point most favorable for absorption, and yet still considerably above that of the freezing point of water, the supply of water is so much diminished that the withering of the leaves is the natural result. Fifth, Pfeffer has pointed out the interesting fact that although living protoplasm resists the entrance of colored liquids of even moderate strength of solution, it will permit very dilute coloring agents of some kinds to pass into its substance and even to enter the sap cavities. I find that there are not only differences in regard to the absorption of the same liquid at different temperatures, but also of the same liquid by different plants and especially the plants of different climates at the same temperature. For the purpose of this later study, it has been found convenient to use the apparatus described by me in a communication read before the Academy two years ago, and which has since that time found elsewhere extended application.

There is a curious embarrassment attending the selection of material for these studies which I have felt from the outset: Is it not likely that some of the plants cultivated by us in our hot-houses as tropical, and which came originally from the tropics, have become more or less modified through adaptations to their new surroundings? At any rate, I am not yet prepared to deny that here may be an element of uncertainty when we apply the results of this research to the vegetations of tropical plants in their homes. Again it must not be forgotten that we make little distinction in our hot-houses between those tropical plants which grow in jungles where they may be sheltered more or less from radiation, and those which grow in the moist plains under an open sky. For subjecting the tissues of the plants to varying temperatures a special apparatus has been devised by me which forms the subject of a separate communication. G. L. G.

3. *On an Apparatus for easily controlling temperatures at or below freezing, for experiments on the relations of plants to cold.** —The ingenious device by which, through the intervention of a refrigerating coil, it is possible to make excavations in sandy and treacherous land, gave the writer a hint as to the construction of a simple instrument for subjecting plants to low temperatures. Expressed in its lowest terms, the apparatus consists of a metallic or glass tube running round the inside of a properly arranged box adapted to the reception of a microscope. Through this coil there is made to circulate by means of a pressure bulb, a current of a dense solution of calcium chloride kept at the desired temperature. This solution can be carried down far below the freezing point of water, and it is of course capable of being kept perfectly under control. In this way, the temperature of the interior of the box falls after a short time to the desired degree, and it can be kept at this point for any length of time. It is to

* Read before the American Academy, Nov., 1889.

be said that the changes in temperature take place ordinarily very slowly, and this has been found to be a distinct advantage.

The apparatus can be employed for the examination of living plants or microscopic sections, as in both cases the changes of temperature succeed each other so slowly that one can follow the effects produced by them without any difficulty. G. L. G.

4. *The disintegration of woody tissues.*—A review of all the processes hitherto published has convinced the writer that the method most widely applicable for the separation of the structural elements of hard vegetable tissues, is the following, which has been in use in the Botanical Laboratory of Harvard University for several years. The tissue is soaked for a sufficient length of time in a ten per cent solution of potassium dichromate, then quickly freed from the excess of the salt by once rinsing in pure water, and immediately acted on by concentrated sulphuric acid. After the acid has acted for a short time, the tissue is to be placed in a large quantity of water, when it will be found to have undergone a more or less complete disintegration, which has left the constituents practically uninjured. When this process, which is really a chromic acid method, is correctly used, there is merely a separation of one structural element from its neighbors, with little or no corrosion of the wall. It has been found easier and far more pleasant to employ than the macerating method in which potassium chlorate and nitric acid are heated together. Moreover, one obtains all the excellent results which can be gained from the most cautious employment of the chlorate method.

Mr. Stone, of the Worcester Natural History Society, has shown me that the process is readily applicable to even such tissues as Collenchyma. G. L. G.

5. *Illustrations of West American Oaks, by the late* ALBERT KELLOGG, M.D., the text by Edward L. Greene, San Francisco, 1889. 4to, pp. 47.—By an appeal to James M. McDonald, Esq., of San Francisco, Professor George Davidson secured the funds for the publication of Dr. Albert Kellogg's drawings of the Oaks of California. These excellent illustrations, explained by text by Professor Greene, of the University of California, are now before us, and they justify the forcible and discriminating appeal which met with so prompt a response. The drawings are in outline, with very little shading, and give the chief diagnostic features with much distinctness. That they are truthful in every detail must be believed by all who knew Dr. Kellogg. Of this enthusiast, whose drawings of the Oaks are happily saved to us, his friend Professor Davidson says, in the introduction to this work, p. vi, "It was the unselfish and successful work of Kellogg and his colleages through twenty years that educted the first munificent gift of James Lick, and the second still greater one. It was his devotion that subsequently elicited the noble gift of Charles Crocker for the endowment of original research . . . As Dr. Kellogg's years gradually increased, the field of investigation seemed to expand a hundred fold, and again his singleness of

purpose asserted itself. He forsook his profession to devote his life to botany; he forgot where the raiment, the sustenance and the house-protection were to come from. He faithfully believed that his other self, Harford—just as devoted and needful as himself—would see to it that he was clothed for the benefit of his fellow men. For the rest, his time was no longer his own; he gave it unreservedly for the benefit of his fellow-men. His pencil and his pen were never afterwards out of his hands while daylight lasted." It is sincerely to be hoped that the other illustrations left by Dr. Kellogg may find their way into botanical literature in the unexceptionable form in which these have been given to the world. G. L. G.

IV. Miscellaneous Scientific Intelligence.

Ascent of a Peak in the Cascade Mountains. Dr. Julius Röll, of Darmstadt, in the course of a mission of botanical exploration in the north-west of America, made an ascent in June, 1888, of a summit in the Cascade Mountains, hitherto unnamed on our maps. The peak in question is situated under long. 121° 15′ W., and lat. 47° 22′ N., between two small lakes, and about 20 miles north of Easton on the Northern Pacific Railroad. We take the following from a short account of his excursion contributed by Dr. Röll to the current number of Petermann's 'Mittheilungen.' On the 19th June, in company with Herr Purpus, he made his way through the primeval forest, and over rising ground to the foot of the mountain, pitching his tent at an altitude of 5,500 feet. The next morning the actual summit was ascended. It is composed of melaphyr, and many pieces of agate and rock crystal were found. The steep slopes are overgrown with ceanothus bushes, maples, and pines, between which bloom yellowish-red lilies (*Lilium philadelphicum*), and species of dark-red pentstemons. Three successive summits were climbed, the highest was estimated at 7,500 feet; unfortunately the exact altitude could not be ascertained, as the traveller's barometer had become useless. The rocky crest of the mountain is covered with the *Selaginella rupestris*, pentstemons, phlox, pedicularis, several saxifrages, and some low umbelliferous plants, &c. Traces of bears, moose, and mountain sheep were observed. The following day another ascent was made, and a magnificent view of the snow-covered Mount Tacoma obtained. Some weeks later, finding that the peak he had ascended was unnamed, Dr. Röll designated it "Mount Rigi," from the resemblance to the Swiss mountain of that name.—*Proc. R. Geogr. Soc.*, Oct., 1889.

A Bibliography of Geodesy by J. Howard Gore, B.S., Ph.D. pp. 315–512. U. S. Coast and Geodetic Survey. Appendix No 16, Report for 1887. Washington, 1889. A very valuable and complete work compiled as the result of a vast amount of labor involving the exploration in person of thirty-four of the principal libraries of America and Europe and many minor libraries by proxy, besides an extensive correspondence.

APPENDIX.

ART. XIV.—*Description of New Dinosaurian Reptiles;* by O. C. MARSH. (With Plate I.)

RECENT explorations in the West have resulted in the discovery of many remains of Dinosaurs, some of which are of more than ordinary interest. A few are from the Jurassic, but most of them are from the Cretaceous, especially from the upper portion, in the so-called Laramie formation. Those found in the latter horizon show a high degree of specialization, and present some anatomical features not before observed in this group of reptiles. Several of the new forms are briefly described below, and will be more fully discussed in a later communication.

Triceratops serratus, sp. nov.

First in importance of the new discoveries is a nearly perfect skull of the genus *Triceratops*, a typical example of which (*T. flabellatus*) was described and figured by the writer in the last number of this Journal.* The present skull is more perfect than any hitherto found, and exhibits admirably the strongly marked characters of the genus. It is likewise of gigantic size, being nearly six feet in length ($1 \cdot 8^{m}$), although the animal was not fully adult.

A striking peculiarity of this skull, which has suggested the specific name, is a series of bony projections on the median line of the parietal crest. The latter is elevated along this line to support them, and the sides descend rapidly to their union with the squamosals. There is a second series of elevations along the middle of the squamosal bone as it falls away from the base of the horn-core, but these are much less prominent.

The orbit is nearly circular in form, instead of oval, and is situated above, and forward of its position in the species referred to. The quadrato-jugal meets the anterior process of the squamosal, forming a closer union than in the skull previously figured. In this respect, and in the elevations on the squamosal, it approaches a much smaller specimen at present referred to the genus *Ceratops*.

*This Journal, vol. xxxviii, pp. 501–506, December, 1889.

The nasal horn-core is wanting in the present specimen, as it was not ossified with the nasals. It projected upward and forward. The nasal bones extend outside the superior branch of the premaxillaries, the lateral suture uniting the two being nearly vertical.

The present specimen is from the Ceratops beds of Wyoming, in essentially the same horizon of the Laramie as the skull of *Triceratops flabellatus*, to which reference has been made.

Triceratops prorsus, sp. nov.

A second skull of this genus, fully adult, and of nearly equal dimensions, was secured at the same time as the specimen last described. It is in excellent preservation, although somewhat distorted, and evidently belongs to a distinct species.

The nasal horn-core and the rostral bone are in position, and perfect. The former is very large, and is directed straight forward, its upper surface being nearly on a line with the superior face of the nasals. It is somewhat oval in transverse section, and pointed in front, the apex being directly above the anterior extremity of the rostral bone. It is so firmly coössified with the nasals that no trace of a suture can be observed. Its external surface is rugose from vascular impressions, indicating that it was covered by horn, thus forming a most powerful weapon.

The huge frontal horn-cores are more massive, and less slender, than in the species above described.

The parietal crest is not so broad as in the two species last described, but appears to resemble more strongly that of *Triceratops horridus*, its sides being inclined downward, as if to protect the neck.

The rostral bone, likewise, is very similar to that in the last species, but is somewhat more compressed. The two forms may be readily distinguished by the nasal horn-core, for in *T. horridus*, this is comparatively small, and points directly upward, instead of straight forward, as in the present species.

With this skull were found several cervical vertebræ, and some other portions of the skeleton. The atlas, axis, and third vertebra are firmly anchylosed with each other, and their ribs, also, are coössified in the same mass. This union, unknown hitherto among the *Dinosauria*, was evidently rendered necessary to afford a firm support for the enormous skull. The remaining cervical vertebræ are short and massive, and the articular faces of the centra are concave or nearly flat.

The present specimen is from the Laramie of Wyoming, and was found in the same vicinity as the skull above described.

Ceratops paucidens.

The specimen recently described by the writer under the name *Hadrosaurus paucidens** should probably be referred to the genus *Ceratops*, as a comparison with more perfect specimens indicates a much closer affinity with that genus than at first supposed. In addition to the maxillary described, one of the premaxillaries is in good preservation. This agrees in general features with the corresponding bone in *Triceratops*, but is less specialized. Its inner surface is deeply concave, showing that the two premaxillaries did not meet each other closely, as in *Triceratops*, but apparently only in front. This species, as well as the type of the genus, *Ceratops montanus*, represents smaller, less specialized forms of the family, and may be from a lower geological horizon than the gigantic reptiles which the writer has recently made known.

In addition to the special characters of the *Ceratopsidæ* shown in the skull, as stated by the writer in this Journal (vol. xxxviii, p. 505), the following features seen in other parts of the skeleton may be mentioned:

(1) The atlas and axis, and one or more adjoining cervical vertebræ are coössified with each other.

(2) Their cervical ribs are likewise firmly united with the same vertebræ.

(3) The remaining cervical vertebræ are short, and have the articular faces of the centra nearly flat.

(4) The trunk vertebræ have very short centra, with flat articular ends. Above the centra, they resemble the vertebræ of *Stegosaurus*.

(5) The sacrum was strengthened by union with several adjacent vertebræ.

(6) The caudal vertebræ are short and rugose, and the tail was of moderate length.

(7) The ilium is elongated, especially in front; the ischium slender, and directed backward.

(8) The pubis extended forward, and its posterior branch was wanting.

(9) The limbs were short and massive, and all four were used in locomotion.

(10) The feet were all provided with broad hoofs, as in *Stegosaurus*.

(11) The bones of the skeleton all appear to have been solid.

(12) Dermal ossifications were present, and some species were protected by heavy armor.

* This Journal, vol. xxxvii, p. 336, April, 1889.

Ornithomimus velox, gen. et sp. nov.

The high degree of specialization in the reptiles above described has a partial parallel in a small group of typical *Ornithopoda* from the same horizon. Various specimens of these, recently secured, represent a distinct genus and several species. The most marked characters already determined are manifest in the limbs and feet, and these have been selected for description in the present notice. A typical example is shown on Plate I, figures 1–3, which is the type specimen of the species here described.

On the distal part of the tibia represented in figure 1, the astragalus is seen in place, with a very large ascending process, larger than in any dinosaur hitherto known. The calcaneum is also shown in position, but the slender fibula is absent. This bone was complete, but of little functional value. The tibia and all the larger limb bones were hollow, with thin walls, as indicated in the section, figure 1, C.

In figure 5, the corresponding parts of a young ostrich are shown for comparison. The slender, incomplete fibula is in place beside the tibia. The astragalus with its ascending process, and the distinct calcaneum, are also shown in position. The almost exact correspondence of these different parts in the bird and reptile will be manifest to every anatomist.

The most striking feature of the foot belonging with the reptilian tibia is shown in the metatarsals represented in figure 2, A. These are three in number, and are in the same position as in life. They are the three functional metatarsals of the typical *Ornithopoda* and of Birds. The distal ends of these bones correspond in size and relative position in the two groups, but here, in the present specimen, the reptilian features cease, and those of typical Birds replace them. In all the reptiles known hitherto, and especially in Dinosaurs, the second, third, and fourth metatarsals are prominent in front, at their proximal ends, and the third is usually the largest and strongest. In birds, the place of the third is taken above by the second and fourth, the third being crowded backward, and very much diminished in size.

This character is well shown in figure 6, which represents the second, third, and fourth metatarsals of a young turkey, with the tarsal bones absent. In the reptilian metatarsals seen in figure 2, the same arrangement is shown, with the tarsals in place. The second and fourth metatarsals have increased much in size in the upper portion, and meet each other in front.

The third metatarsal, usually the largest and the most robust throughout, here diminishes in size upward, and takes a subordinate, posterior position, as in birds. The correspondence between the metatarsals of the bird and reptile are here as strongly marked as in the tibiæ and their accompanying elements, above described.

In figure 3, the three phalanges represented belong with the second metatarsal, and were found together in place.

The three metacarpals represented in figure 4 were found together in position, near the remains of the hind limb here described. Their very small size indicates that they may possibly belong to a smaller individual, but, with this exception, there is no reason why they do not pertain to the same specimen as the hind foot.

The remains of the present species here described were found in the Ceratops beds of Colorado.

Two other species, apparently of the same genus, are represented by various specimens from the same horizon, in Montana. One of these, which may be called *Ornithomimus tenuis*, was about twice the bulk of the present form. The third metatarsal was much more compressed transversely, both in the shaft and distal end. The bone was also much more slender medially than in the above species. The transverse diameter of this metatarsal at its distal end was 30^{mm}., and the antero-posterior diameter, 35^{mm}.

A third species, much larger, may be called *Ornithomimus grandis*. The third metatarsal was about 600^{mm}. in length, and its distal end 90^{mm}. in transverse diameter, and 80^{mm}. in antero-posterior diameter.

These various remains represent a distinct family, which may be called the *Ornithomimidæ*.

Barosaurus lentus, gen. et sp. nov.

A new genus of the *Sauropoda* is indicated by various remains of a very large reptile secured by the writer during the past season. The most characteristic portions examined are the caudal vertebræ, which in general form resemble those of *Diplodocus*. They are concave below, as in the caudals of that genus, but the sides of the centra are also deeply excavated.

In the anterior caudals, this excavation extends nearly or quite through the centra, a thin septum usually remaining. In the median caudals, a deep cavity on each side exists, as shown in figures 1 and 2, on page 86.

On the distal caudals, the lateral cavity has nearly or quite disappeared. All the caudal vertebræ are proportionally shorter than in *Diplodocus*, and their chevrons have no anterior projection, as in that genus.

FIG. 1. FIG. 2.

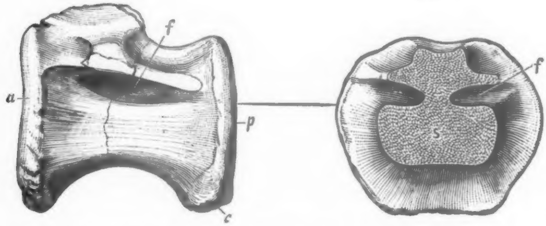


FIGURE 1.—Caudal vertebra of *Barosaurus lentus*, Marsh; side view.
FIGURE 2.—The same vertebra, in section; front view.
a, anterior end; *c*, face for chevron; *f*, lateral cavity; *p*, posterior end; *s*, section.
Both figures are one-eighth natural size.

The remains on which the present description is based are from the Atlantosaurus beds of Dakota, about two hundred miles further north than this well-marked horizon has hitherto been recognized.

For important aid in securing the fossils above noticed, the thanks of the writer are due to Mr. J. B. Hatcher, Dr. C. E. Beecher, and Mr. G. L. Cannon, Jr. The type specimens will be more fully described and figured by the writer under the auspices of the U. S. Geological Survey.

New Haven, Conn., December 21, 1889.

EXPLANATION OF PLATE I.

FIGURE 1.—Left tibia of *Ornithomimus velox*, Marsh; A, front view; B, distal end; C, transverse section.

FIGURE 2.—Left metatarsals of same specimen; A, front view; B, proximal ends; C, transverse section; D, distal ends.

FIGURE 3.—Phalanges of second digit of same foot; front view. A, first phalange; B, second phalange; C, third, or terminal phalange.

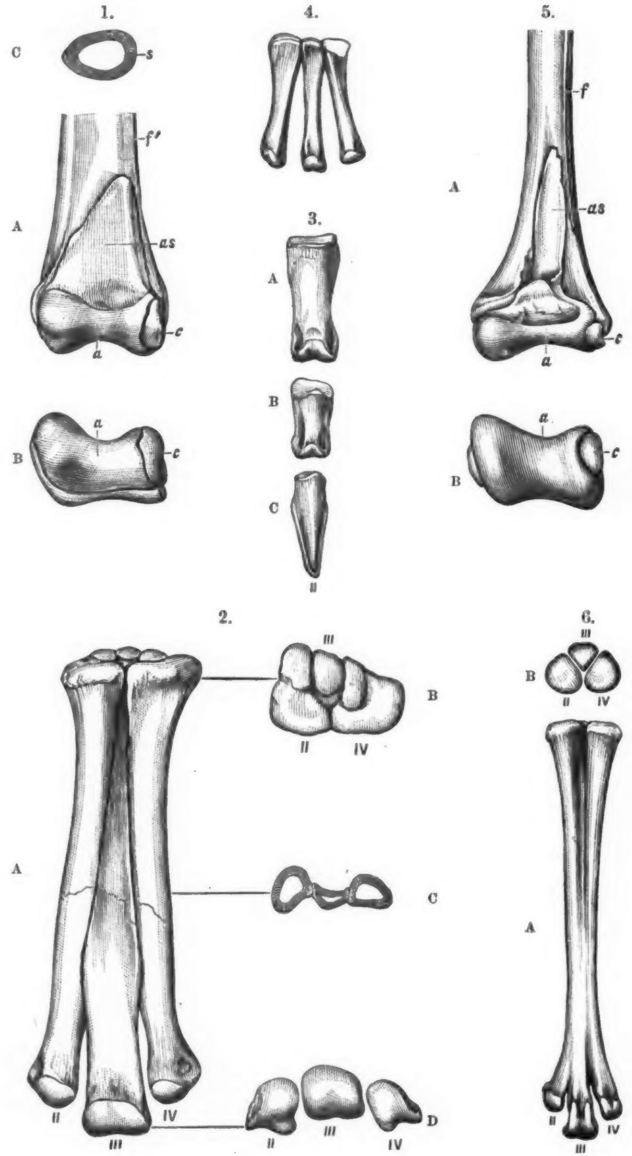
FIGURE 4.—Left metacarpals of same species, perhaps of smaller individual; front view.

FIGURE 5.—Left tibia of young Ostrich (*Struthio camelus*, Linn.); A, front view; B, distal end. The separate calcaneum was first observed by the writer's assistant, Dr. G. Baur, who prepared the specimen.

FIGURE 6.—Left metatarsals of young turkey (*Meleagris gallipavo*, Linn.); A, front view; B, proximal ends.

a, astragalus; *as*, ascending process of astragalus; *c*, calcaneum; *f*, fibula; *f'*, face for fibula; II, second metatarsal; III, third metatarsal; IV, fourth metatarsal.

Figures 1–4 are one-third natural size, and figures 5 and 6, one-half natural size.



Figures 1–4, Ornithomimus; 5, Struthio; 6, Meleagris.

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